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THE EDINBURGH MEDICAL AND SURGICAL JOURNAL,

No. XC.

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Bradshaw's Companion to the Continent: a descriptive Handbook to the chief places of resort; their characteristic features, climates, scenery, and remedial resources; with observations on the influence of climate and travelling. By Edwin Lee, Honorary and Corresponding Member of the principal Continental Medical Academies and Societies, Author of the Baths of Germany, &c. London, Manchester, 1851. Post 8vo, pp. 408.

Physiological Researches. By Sir Benjamin Brodie Bart., D.C.L.F.R.S.; Corresponding Member of the Academy of Sciences of the Institute of France. Collected and published from the Philosophical Transactions. London, 1851. 8vo, pp. 146.

Report of the Proceedings of the Pathological Society of London. Fifth Session, 1850-51. London, 1851. 8vo, pp. 196. Three Lithograph Engravings.

The Pocket Formulary, and Synopsis of the British and Foreign Pharmacopœias: comprising standard and approved Formulæ for the Preparations and Compounds employed in Medical Practice. By Henry Beasley. Fifth Edition. Corrected, enlarged, and adapted to the last editions of the Pharmacopœias. London, 1851. 12mo, pp. 546.

Guy's Hospital Reports. Second Series. Edited by Edmund Lloyd Birkett, M.D. Cantab.; Fellow of the Royal College of Physicians; Physician to the City of London Hospital for Diseases of the Chest, and Curator of the Museum of Guy's Hospital; and Alfred Poland, Fellow of the Royal College of Surgeons; Assistant-Surgeon to Guy's Hospital, &c. Volume Seventh. Part Second. London, 1851. 8vo, pp. 265-466.

On the Transmission from Parent to Offspring of some Forms of Disease, and of Morbid Taints and Tendencies. By James Whitehead, M.D., F.R.C.S., on the Medical Staff of the Lying-In Hospital, and Lecturer on Obstetric Medicine at Manchester, &c. &c. &c. London, Manchester: 1851. 8vo, pp. 351.

Lectures on the Physical Diagnosis of Diseases of the Lungs and Heart. By Herbert Davies, M.D., Fellow of the Royal College of Physicians; Senior Physician to the Royal Infirmary for Diseases of the Chest; Assistant-Physician to, and Lecturer upon Materia Medica at, the London Hospital; and formerly Fellow of Queen's College, Cambridge. London, 1851. Post 8vo, pp. 298.

On the Nature and Treatment of the Diseases of the Heart; containing, also, an Account of the Musculo-Cardiac, the Pulmo-Cardiac, and the Veno-Pulmonary Functions. By James Wardrop, M.D., &c., &c. London, 1851. 8vo, pp. 587.

Neuralgia: its various Forms, Pathology, and Treatment. Being the Jacksonian Prize Essay of the Royal College of Surgeons for 1850; with some Additions. By C. Toogood Downing, M.D., M.R.C.I. London, 1851. 8vo, pp. 375.

Translation of the Pharmacopœia of the Royal College of Physicians of London, 1851; with Notes and Illustrations. By Richard Phillips, F.R.S.L. & E., F.G.S.; late President of the Chemical Society; Curator of the Museum of Practical Geology, &c. London, 1851. 8vo, pp. 567. [Received on Monday the 8th December 1851.]

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PART I.

ORIGINAL COMMUNICATIONS.

ART. I.—*Hæmorrhagic Diathesis exemplified in two Brothers; with Remarks on its Hereditary Tendency.* By Dr PICKELLS of Cork.

THESE cases occurred in the country, at a place called Cloghroe, about seven miles from this city. The elder brother, aged about 20, had been reaping during a hot day, in harvest, when on coming to the end of a furrow or ridge, in endeavouring through a spirit of rivalry to out-do his fellow-reapers by being the first to put his sickle in a new ridge—a sort of rivalry often encouraged in their avariciousness by employers or hard task masters; his reaping hook striking against a stone, glanced aside, and wounded him in the wrist. He died of the hæmorrhage on the same day. The younger brother, a lad of about 14, happened in about a year after to cut himself slightly with a pen-knife in the thumb. The wound was bound up in the usual way, and the hæmorrhage appeared to have ceased, when on the third day after the accident, making some slight use of the thumb, it broke out afresh, and he died in the course of the same day.

Regular medical assistance was not obtained in either case; but various simple styptic applications, such as are used in domestic medicine, were resorted to. The elder brother had been placed in a sort of easy rustic vehicle, called a butt, for conveyance to one of the surgical hospitals of this city, but sank before he reached his journey's end. They were both of a thin habit, with a flush or degree of floridity on the cheek, and not unhealthy. They had previously shown no disposition of the kind, nor has

The most remarkable characteristic of the disease is its hereditary tendency. In this respect some of the instances on record possess a fearfully tragic and momentous interest. Thus in a case given, (*Edin. Med. and Surg. Journal*, vol. xxv.), which occurred near Bristol, it was stated by the mother of the child who died, that none of the family had, during some generations, reached to man's state, and that they had all of them been subject to profuse hæmorrhages from the most trifling wounds. Her own brother had died of hæmorrhage succeeding the extraction of a tooth. The females enjoyed good health, and were entirely free from the tendency to bleed excessively from superficial wounds, but if they married their sons inherited from them this family curse, whilst their daughters were equally free with themselves from it. The law in this respect appears to be very singular, a sort of penal (if the term be allowed) salic law, entailing the disease, in its hereditary character, on the male branches of a family to the exclusion of the female; yet transmitting it through the females to the males of the next generation.

In Mr Wardrop's paper, the following case is given on the authority of a medical practitioner in York, as illustrative of the hereditary tendency of this diathesis, in which it was hereditary in the male branches of the same family, and confined to males. "A boy labouring under this diathesis had five uncles and two aunts; all his uncles had the same hæmorrhagic tendency; the aunts had not that tendency in their persons. The one had three boys, two of whom are thus affected; the other has two boys and two girls, both brothers are affected in the same way. The mother of the boy whose case is given "had a numerous offspring, and a brother, who is 22 years, is affected in a similar way, and is an almost constant sufferer from rheumatic gout."

An instance still more striking is that published by Dr Nasse, (occurred in Germany) in *Hufeland's Journal*, as extracted in *Edinburgh Medical and Surgical Journal*, vol. xxv. "A woman, whose paternal uncle had been subject to epistaxis, had lost all her five brothers in early youth by hæmorrhages from trifling injuries, or without ascertained causes. She herself had five sons and two daughters. Of the sons, four had died of hæmoptysis. None of the females of the family had shown any hæmorrhagic disposition.

In article "Hereditary Diseases," in *Cyclopædia of Practical Medicine*, a minute history, taken from a German journal of cases which occurred in Oldenberg, is given by Dr Riecken. The parents had never been subject to hæmorrhage; and the father, Ernest P., was living in good health, in his 88th year, at the time of the publication of the narrative. The couple had twelve children, five sons and seven daughters, of whom three boys and one girl died of hæmorrhage. Their youngest daughter, who never

and the development or manifestation of the disease may be thus prevented.*

When fatal, it has often terminated with frightful rapidity,—life ebbing within a few days from commencement of attack; in the instance of the Cloghroe brothers, within a day or two. In other instances the disease has been protracted, the hæmorrhage continuing to recur at intervals more or less distant for months or even years. Of this protracted form are examples, a case in Part Second of Boates's *Natural History of Ireland*, entitled "*Periodical Evacuation of Blood at the End of the Forefinger*," and another in Vol. v. of *Philosophical Transactions*, abridged, entitled "*An Extraordinary Periodical Hæmorrhage from the Thumb*." These cases have not, I believe, been noticed in medical works. In the first-mentioned, the patient was an inn-keeper in Trim, county of Meath, of sanguine complexion. With him the hæmorrhage did not begin till the forty-third year of his age (coming on about Easter), but continued to recur at frequent intervals, seldom giving a respite of two months during the remainder of his life, which was prolonged to twelve years. The man met with no outward accident which might at first have brought it on him, it having been a spontaneous act of the constitution. He rarely bled less than one pottle at a time. The blood would spin out from the end of the forefinger, the right, in a violent but small stream. After it had spent its violence, it would cease for a time and then only drop, then spring out again with violence, continuing thus for twenty-four hours, till at last he fainted away, when the blood stanch'd of itself, and his pains left him. Drinking more than ordinarily made him more apt to bleed. He died of the disease.

This case confirms a statement of Dr Hay, that age does not exempt. The other case was that of a person who, from his earliest years, even, as he heard, from his infancy, had been subject to the hæmorrhage (from the right thumb), the time of the eruption being about the full of the moon. Under this discharge, however copious, (when he came to be sixteen, the quantity had increased to half a pound at each eruption), he was strong and vigorous to the age of twenty-four. At that age, finding the evacuation troublesome, and being uneasy under it, he seared

* Among diseases which run in families, may be included, there is reason to apprehend, that direst of maladies, the Asiatic cholera is to be placed. A clergyman, now residing in the neighbourhood of this city, but then residing in a distant part of the county, informed me, that, in 1832, he knew an instance of four members of a family living in different places, having been all attacked with cholera without the slightest communication with each other. In Persia, I have somewhere seen it stated, certain families of the nobility doomed, as it were, by a peculiar constitutional liability, have been rendered extinct, or almost extinct, by the malady, which, from its repeated visitations, threatens to become domiciliated among us.

with a hot iron the orifice which used to give it vent. The searing had stopped the hæmorrhage for about twenty years. The consequences were, however, very serious, for within one quarter of a year after the operation, he was seized with severe hæmoptysis, spitting up vast quantities of blood by coughing. This was arrested by frequent blood-letting, &c., yet not with that relief which was expected; for, in a very short time after, a most violent colic supervened,—a disposition to which, though in a good measure overcome by purgatives, still remained at the date of the account, being often troubled with it, as also with a spitting of blood on the least excess of cold or motion.

From this and other instances of such like hæmorrhages in men, the medical reporter, Dr Musgrave, was led to infer that these evacuations, being analogous to the *menses* in women, confirm the opinion of all such as derive that discharge from a *fermentum uterinum*. “For,” he asks, “how can we think the *menses* come from such a local ferment, when a discharge in all respects equivalent to them takes place where no such ferment is, or can be supposed?” Of late years, the doctrine has been broadly maintained by Gall of “a periodic movement in the male system, analogous to that which returns monthly in the female;” “the hæmorrhage,” according to Chomel, “recurring sometimes at precisely regular intervals, and by monthly periods more commonly than any other; its accidental interruption being almost uniformly the cause or the effect of some derangement of the health.”*

In the protracted cases now instanced, the hæmorrhage was confined to one part (the finger). In the volume already referred to of the Philosophical Transactions is a very remarkable case by Dr Mesaporiti of Genoa, entitled, “An Eruption of Blood from Several Parts of the Body.” Among other parts, the blood oozing or escaping, as has happened in some, though rare, cases of the hæmorrhagic diathesis, even by the pores of the skin (hæmorrhage by exhalation). The original is in Latin (*ex utrâque velaminum, et ex opposita parte*.)

A young lady, aged eighteen, of singular beauty, after a stomach complaint of long standing, began early in April to spit up blood by coughing, labouring under the pleuritic symptoms of pain in side, and difficult respiration. On the fourth day of the attack, the pectoral affection ceasing, epistaxis set in, for which the surgeon took blood from the foot, as he had on the first days of the attack twice taken it from the arm. The disease not

* There is an apparent forcing of facts, procrustes-like, by Chomel, to suit his hypothesis. The straining of isolated facts, in pursuit of an hypothesis of this sort, might seem to some to merit the sarcasm applied by Reid in his essays to certain theorists, as, in their neomania, or love of paradox, “Hunting after bladders of soap in the land of chimera.”

abating, vomiting, first of bile, and afterwards of blood, came on; the constant vomiting and loss of blood being most distressing. To meet this symptom, besides internal means resorted to, including astringents and anodynes, blood was taken from the *vena salcatella*. The disease, notwithstanding, still proceeding, the symptoms now mentioned were exchanged about the beginning of June for a new and complicated train,—bleeding from the ears, from the tops of the fingers and toes, from the navel, from the angle of the eye; several times by sweat; then from the middle of the chest; afterwards from the foot, there where the saphene vein is pressed on (*tunditur*); lastly, from both wrists, and the parts opposite; from the chin, and, during nights, from the tip of the tongue. These anomalous and erratic symptoms all completed their course within the space of a fortnight. About the middle of June, expecting benefit from change of air, she removed to the country, but returned early in August without amendment, or even worse; the hæmorrhages still taking place, sometimes from one part, sometimes from another, but most frequently by sweat, during the remainder of that month, and up to the middle of September, towards the end of which month the symptoms had all much abated or ceased. Thus the disease, with occasional intermissions, in one instance of a week, in another of twenty days, at the time of menstruation (the menses not having been suppressed), had continued nearly six months; the hæmorrhages during that period having been from so many and so various parts, that she may be almost said, without hyperbole, to have “bled from every pore.” The symptom latterly found most distressing was the partial sweating of blood; her chemise, or inner garment (*indusium*), used to be stained from the dew of blood which exuded on the skin, particularly over the chest. What renders this case the more remarkable in a physiological point of view is, that the menses had been always regular and in sufficient quantity; the rare forms of hæmorrhage, and those of the skin in particular, being, in almost every instance, vicarious of, or supplemental to, suppressed menstruation,—a revulsion taking place, and the congestion finding vent by some devious outlet, as in an instance mentioned in one of the volumes of the London Medical Gazette, in which a young girl, who had never menstruated during a paroxysm of ague, sweated blood by the feet.

On looking over reports of cases of the hæmorrhagic diathesis, the reader is struck by its frequent combination with ecchymoses, a combination such as might at first view lead to the supposition of its being identical with *purpura hæmorrhagica*. In vol. xxvi. of the Edinburgh Medical and Surgical Journal, is an article by Alexander Murray, surgeon at Alford, entitled “Cases of Hæ-

Dr Mead, in his Essay on the Small-Pox, arguing from the analogy of the symptoms in "the bloody small-pox" to those produced by the bite of the Lybian serpent, *hæmorrhais*, so graphically described in the passage of Lucan, infers it as evident that "these horrid symptoms in the small-pox are the effects of an acrid poison,"—a species of reasoning which might apply with equal force to the etiology of the hæmorrhagic diathesis, the appalling phenomenon sometimes taking place, as in the bloody small-pox, of hæmorrhages from all the organs. The beautiful passage of Lucan, turned into English, is thus :—

"A fierce hæmorrhais struck both his fangs
Deep into Tullus,—a brave, valiant youth,
And fond admirer of great Cato's worth.
And as Corycian saffron, when 'tis squeezed,
Pours forth its yellow juice through all the holes
Of the hard pressing boards; so from the pores
Of all the parts flowed ruddy-venomed gore.
His tears were bloody; nature's passages,
For their own humours were all filled with blood.
His mouth, his nose, choked up with filthy clots:
Red sweats transpired from all the skin inflamed.
His body seemed one universal wound."

Similar effects are ascribed to the bite of the rattle-snake, sometimes copious bloody sweats, often hæmorrhages from the eyes, nose, and ears.

The poison of the *serpens hæmorrhais* was, however, it would appear, not invariably fatal nor incurable, Celsus having prescribed a formula for the cure of it. Climate would, however, make much difference in this respect; the poison of serpents being most noxious in very hot and arid climates. Sallust, in his history of the Jugurthine war, speaking of an African town, to the siege of which the Romans were about to march, says, "It was more impregnable by its situation than by its walls, and arms, and men, with the exception of the immediate vicinity of the town, all else a vast desert, without water, and infested by serpents; the violence of which, as of all wild animals, is rendered fiercer by want of food. Add to this the nature of serpents, deleterious of itself, is more inflamed by thirst than by any other thing." The symptoms of hæmorrhage from the mouth, and ecchymoses on the skin, are often, too, it is stated, the effects of the poison of hemlock. How much the action of poisons on the human economy is modified by a full or empty state of the stomach, is pointedly exemplified in the circumstances of the death of the twenty-eight senators of Capua, who, in the second Punic war, poisoned themselves, as recorded by Livy, rather than fall into the hands of the Romans, incensed by the revolt of that city to Annibal. "At a feast prepared at the house of the senator

who had been the author of the revolt, after they had been satiated with meat and wine, a cup, containing a poisonous potion, was carried round. With minds alienated, so far as possible, by wine, from the sense of impending evil, all took the poison (hemlock, it is to be presumed)." * * * * "The veins, replete with the meats and wine, rendered the force of the poison less efficacious in hastening death. Most of them lingered throughout the night and part of the following day. All, however, before the gates were opened to the enemy, expired."

In mentioning hemlock, I take the opportunity of correcting an inaccuracy, into which I find I fell in an article published in a former volume of this work (vol. lxvii.), entitled "*Deleterious Effects of *Ceanothus Crocata*, or Hemlock-Water Dropwort, as it occurs in the south of Ireland.*"

Speaking of the *Atropa Belladonna* (deadly nightshade) now fortunately become very rare, I said it was to be found at the East Ferry in the Harbour of Cork. I should have said the *Solanum Nigrum* (black nightshade).

ART. II.—*History of a Case of Epilepsy, accompanied with Fever, Inflammation, Perforation, and Gangrene of the Lungs, and also Pneumothorax.* By ROBERT SCOTT ORR, M.D., formerly Resident Superintendent of the Glasgow Royal Infirmary.

JANET DUNCAN, aged 24, was admitted into the Royal Infirmary, in the month of December 1839, at which time I acted as clinical clerk to Dr Alison.

From what could be ascertained from her friends at that period, and subsequently, the following appears to have been the correct history of her case.

About ten years before her admission into the hospital, she was attacked in the middle of the night with a fit, which, from the account given of it, and the frequent recurrence of similar fits afterwards, seemed to have been of the nature of epilepsy. She was for about a year after this afflicted with a fit generally once a month; there was at that time never more than one in number, and it lasted generally only about one minute; previously and subsequently to its occurrence she suffered a good deal from headache. During the fit her limbs were convulsed; her eyes turned upwards; her tongue was clenched between her teeth, and was frequently severely bitten; she moaned and sometimes cried, after which she always fell asleep; and next morning (for these attacks always occurred at night) she was unconscious of having been ill.

After she had been thus subject for about twelve months to these attacks, an interval of seven years elapsed, during which she remained quite free from them ; the year following that, however, she was again in a similar manner affected.

For twelve months before she came into the hospital the catamenia had been very scanty and irregular ; and she complained much of headach, with latterly sense of tightness across the forehead, and appearances as if balls of fire were passing before the eyes. The fits at this time also had increased in the frequency of their attacks, occurring once a fortnight ; but subsequently to the application of leeches to the head, they supervened only once in six weeks.

The week before her admission she had three fits each night for three successive nights ; and on the night of the 11th of December she slept little, and complained much of pain in the head. On the morning of the 12th she ran away from home, and came into the Infirmary in quite a maniacal state, complaining of uneasy feelings, and crying to have the head shaved and blistered.

She was accordingly put under the care of Dr Alison, who directed that she should be bled from the arm to the extent of twelve ounces ; that she should take half a grain of tartrate of antimony, dissolved in one ounce of water, every hour, and that cold applications should be applied to the head.

The following reports were taken by Dr Alison of her state and symptoms while she remained in the house.

" *December 13th.* Twelve ounces of blood quite natural. Had several doses of antimonial solution, and soon became quiet and taciturn. One scanty stool from powder of jalap and calomel. The pulse was of natural frequency, rather small ; the face is pale ; skin cool ; no vomiting.

" Cold applications to the head to be continued ; and if stupor persists, a blister to be applied over the vertex in the evening.

" *14th.* Blister was applied last night, and had two pills and a senna draught this morning. Blister has risen and medicine operated. Speaks sensibly this morning. Pulse 78, soft. No spasmodic symptom.

" Continue antimonial solution, if delirium returns.

" *15th.* Bowels well moved again last night ; had a quiet night. Pulse rather slow ; takes a little food ; tongue dry.

" One ounce of Epsom salts to be dissolved in eight ounces of water, with one drachm of dilute sulphuric acid, and one ounce of the solution to be given every third hour until the bowels are moved.

" *16th.* Had a quiet night ; medicine has not operated. The solution of sulphate of magnesia to be continued.

" *17th.* Medicine operated ; head is easy ; slept quietly.

The solution of sulphate of magnesia to be repeated to-morrow morning.

"18th. Had a quiet night. No headach to-day. Is still taciturn. The acid solution of Epsom salts to be continued; and the feet to be placed in warm water in the evening.

"19th. Had an easy night; no headach; pulse natural; still very taciturn.

Over the head to be applied a lotion of tepid water and vinegar. To take two of the pills of aloes and iron twice daily; and the feet to be placed in warm water in the evening.

"20th. Had a quiet night. Bowels open. The pills of aloes and iron to be continued.

"22^d. Pediluvium to be repeated, and pills to be continued.

"23^d. Had some headach last night, and slept little; pulse natural, 78; bowels open.

"Eight leeches to be applied to the temples. To have, at the hour of rest, a draught containing one drachm and a-half of tincture of hyoscyamus.

"*January 4th, 1840.* Continues easy and sleeps well. Bowels kept open without pain."

She was now dismissed with directions; and was recommended to take two of the pills of aloes and iron twice daily. Of these she received at dismissal three dozen. She was dismissed much relieved, not having had any return of the fits since the time of her admission.

On the 8th of January I was requested to visit her at her own home, and found her much in the same state, in which she was when she left the hospital.

On the 15th I again saw her. The catamenia had not then returned, and indeed they never afterwards did return. She had had a fit each of the previous nights, lasting about one minute; and had since that occurrence complained of headach; although I had never seen her during a fit, still I could not doubt that they were truly epileptic. Notwithstanding the supervention of these attacks, she was ordered to continue the use of the pills of sulphate of iron and aloes, and had no more return of them till February 4th, when she had a slight fit.

For above seven months she continued pretty well, with, however, occasional fits at intervals. But on the 11th of September she had a number of severe attacks, amounting, it was stated, to ten or twelve. Dr Alison saw her and bled her; ordered tartar emetic ointment to be rubbed on the head; gave her tartar emetic internally, and also pills, containing extract of colocynth, hyoscyamus, and croton oil. I had been previously sent for, but not being at home, Dr Alison was gone before I arrived. I how-

ever saw her immediately after he left. The face was then pale; the eyes were heavy; the arms were pretty rigid; and she was quite taciturn and incoherent.

On the 13th she was still taciturn. Two pills produced only one motion. The pulse was 86; the arms were rigid; the tongue was covered with a whitish moist fur. Two more of the pills were given, and the tartar emetic frictions were recommended to be continued. Dr Alison advised the use of small doses of turpentine, and entrusted the patient to my care.

14th. Tongue still foul, and bowels still sluggish.

Two more pills were ordered, and the following mixture:—

Oil of turpentine half an ounce; mucilage of gum arabic four ounces; syrup of orange pill three drachms; water four ounces. To take one ounce of this mixture three times daily.

17th. Had a very restless night, with constant talking and jactitation. Bowels confined; tongue foul; and breath heavy. Is at present restless, with frequent puffing motion of the mouth; limbs rigid, especially left arm. She is quite insensible.

The hair to be removed, and a blister to be applied to the scalp. To take immediately two drops of croton oil, two grains of hyoscyamus, and one grain of compound extract of colocynth in the form of pill.

Vespere. Bowels still unmoved; still restless and talkative; arms rigid; pulse 120, small; tongue foul and dark-coloured.

An enema with one ounce of oil of turpentine to be administered. The antimonial solution to be continued.

18th. Blister rose well; is at present asleep, and has been quite quiet. One motion passed unconsciously. Pulse 88, small and feeble; rigidity of limbs gone; surface of the body warm and perspiring.

A domestic enema to be administered.

Vespere. Is now perfectly quiet. Pulse 76, of better strength. Tongue still covered with a fur, but it is whiter and much moister; it is considerably injured by the teeth. The patient complains of hunger, and is much inclined to sleep; head covered with a copious crop of pustules from the tartar emetic ointment. When asked if she felt better she replied, "Yes," and protruded the tongue when desired.

Enema not given.

19th. Enema not given hitherto. The pulse is more frequent, but she is more rational and can name those around her. One motion. Tongue covered with a white moist fur. She complains still of hunger, although she has had food, and is still much inclined to sleep. She says she is "sick, sick."

A domestic enema to be administered.

20th. She continues sensible, though still somewhat incoherent; tongue furred; two motions from enema; pulse 96.

To have a draught of infusion of senna.

21st. Still somewhat incoherent and restless; pulse 86, of good strength; appetite good; bowels well opened by senna infusion.

Three grains of tartrate of antimony were dissolved in three ounces of water; and of this solution she was directed to take half an ounce every second hour.

23d. Is now perfectly quiet and rational, and understands what is said to her. Pulse 90, of good strength; appetite good; tongue much cleaner; complains now only of some head-ach; bowels open, and she is now conscious when the stools are passed.

30th. Is now quite well and perfectly sensible, her intellect not being seemingly impaired. The appetite is good. She continues the use of the tartar emetic ointment, and attention to diet was strictly enforced.

Nearly five months elapsed after this date before I again heard anything of this young woman. On the 27th of February 1841 I was however sent for to see her, and on my arrival I was told that since the previous day she had had nine fits. She was when I saw her stupid, drowsy, and rather restless; but when roused she answered questions, and complained of headach. The tongue when protruded was furred; the pulse 70, full; the bowels said to have been freely opened by medicine.

I directed the following medicines to be given.

Three minims of croton oil and eight grains of aloes were directed to be made with aromatic confection into four pills; and of these one was to be given every fourth hour till the bowels were well moved.

A solution of tartrate of antimony was also directed to be prepared, consisting of three grains of the salt in six ounces of water, with two drachms of solution of muriate of morphia; and of this a table-spoonful was ordered to be given every two hours while the patient was restless.

That same evening she had three more fits; and when I saw her next day she was still drowsy, but sensible when roused; the pupils were slightly contracted; the pulse 88; the tongue furred and dry in centre, and the headach continued. She had taken only one pill, and that without effect. In the evening, being much in the same state, and another pill taken having produced no effect, the pulse being from 94 to 96, sharp and full, and the drowsiness continuing, I bled her from the arm to the extent of 18 ounces, and afterwards gave her another croton-oil pill, with

directions that, if this failed to move the bowels, an enema, with an ounce and a half of turpentine, was to be administered. The head was also to be shaved.

Next day, March 1st. The blood drawn was slightly cupped, but not buffed. She was, however, still stupid and listless, protruding the tongue when desired, but forgetting to draw it in again. The pulse was 100, much smaller. One stool had been passed consciously. The tongue was nearly clean. The urine was voided unconsciously. She had been rather restless in the night, but had no return of the fits.

A cathartic draught was ordered, and the antimonial solution to be continued.

On the 2d she was still taciturn. The tongue was dry and furred; the pulse 88. There had been no more fits, and there was no rigidity of the limbs. One motion had taken place in the night. A teaspoonful of morphia had been unadvisedly given; she slept well.

A croton oil pill was ordered, and afterwards a cathartic draught. The antimony to be continued.

3d. Taciturnity continues; pill produced one motion; pupils natural; pulse 88; tongue dry in centre, but she forgets to draw it in again; some thirst; she is rather more intelligent.

To two drachms and a half of the compound jalap powder were directed to be added half a drachm of carbonate of soda, and fifteen grains of powder of scammony; the whole to be divided into three powders, one of which was to be immediately given; and if the bowels were not freely moved, to be repeated in the morning.

4th. Much in the same state; pulse 88; one motion passed unconsciously. Two powders taken.

The third powder was ordered to be given, and a blister to be applied over the scalp.

5th. Blister rose well; less taciturn, and answers questions; two involuntary motions; tongue cleaner and moister; pulse 70; slept well; no fits.

To have to-morrow morning one ounce of castor oil.

9th. Quite rational; bowels open; pulse natural.

On the 27th of April I was called to see her. She had had fits at intervals for the two previous weeks. The night before I saw her she had had five. She was restless and drowsy, but answered questions slowly, and said she had no headach. The pulse was about 80, and the tongue foul.

She was directed to continue the use of the croton oil pills and antimonial mixture formerly prescribed.

On the 9th, 10th, and 11th of May she had a fit each day,

16th. Pulse still 128; but she is less drowsy, and opens the eyes; does not however protrude the tongue or answer questions when desired; two motions. On the sacrum there is a dark spot about the size of half a crown, surrounded with a red areola; there is also a spotted eruption on the face; stools and urine since the 13th voided involuntarily.

To have applied over the sacrum the liniment of soap and opium.

In the evening the pulse had fallen to 100, but she was still stupid.

On the 17th she had two motions; the eye was clearer, but she did not answer questions or protrude the tongue. The face was of a more natural colour, but there was some degree of restlessness. No return of the fits since the morning of the 14th. She had a restless night, with frequent crying and vociferation. On the 18th the pupils were natural, but she cried frequently, with choking sensation in the chest and peculiar motion of the right arm. No stool since yesterday morning.

To take two compound colocynth pills at the hour of rest, and one ounce of the antimonial solution, if vociferation continue.

She remained much in the same state on the 19th and 20th. She had a quieter night on the 21st, but without sleep. She still, however, cried hysterically, and seemed to suffer pain from the blistered surface on the head, and also at the sacrum, where there is now a distinct slough, with an ulcerated margin. The pulse on the 21st was 100. There had been no return of the fits, but she was still stupid; not answering questions. There was no rigidity of the limbs; eyes suffused; no motion since the 19th.

A croton oil pill was ordered, with one drop of the oil; and a cataplasm was applied to the slough on the sacrum.

When seen in the evening the pill had not been given as one loose motion had taken place; the pulse was then 112. The skin and scalp were hot.

Cold applications were recommended to be applied to the head.

At this time it was my impression, that besides the epileptic, she was also suffering under a hysterical tendency, and that she was affected with continued fever there could be little doubt. I therefore mentioned her case to Dr Craigie, who most kindly offered to accompany me in my next visit. We accordingly went together on the 22d, and the following was her state at that time.

She had had no sleep for two nights, but she was quieter than she had been; the pulse was 134; the respiration 32, with some tracheal rattle. The eruption was still present on the face, and also on the arms, hands, and even fingers, but did not exist upon the chest. It consisted of small elevated spots. The eyes were suffused. When asked to put out the tongue she said she could not, being the first words she had spoken since the 13th. There

was considerable rigidity of the hands, wrists, and elbows. The bowels had not been moved.

Dr Craigie confirmed me in my opinion, that she was labouring under fever, but also thought, from the state of the respiration, that there was some affection of the chest, although she never had any pectoral symptoms, except perhaps slight cough, which, however, was never more than often accompanies the more mild forms of fever. Dr Craigie therefore recommended one blister to be applied to the chest and another to the head, and one ounce of turpentine with half an ounce of mucilage to be given by the mouth.

In the evening when I saw her the pulse was 128; the superior extremities were still rigid; no motion or vomiting had taken place from the turpentine draught.

An enema, with two ounces of turpentine, was therefore recommended, and the feet to be bathed in warm water at bedtime.

On the 23d the pulse was 120. Two motions had taken place from the enema; she was otherwise much in the same state; the rigidity of the arms continuing. The blister on the head rose well; that on the chest but imperfectly.

At this time, from her low and weakened state, I thought it necessary to allow her a little wine, which she took with some arrow root, and seemed to relish it.

In the evening the pulse had risen to 136, but was feeble. There was, however, less rigidity of the limbs. The eyes were suffused, and there was still some eruption visible.

The turpentine enema, and warm foot bath were again repeated, and four ounces of white wine were allowed.

Two motions took place from the enema. Next day the pulse was 124, feeble, but not small. The respiration was 44, with less tracheal rattle. The arms were still rigid, and she was quite incoherent. She had taken three glasses and a half of white wine between eight o'clock of the night before and ten that morning.

A blister was applied to the chest, and the wine was directed to be continued. In the evening she was much in the same low state, and was with difficulty roused.

She was ordered a glass of wine every hour, and sinapisms were applied to the calves of the legs.

On the 25th and succeeding days the following were the reports.

25th. Between 7 o'clock last night and 10 this morning she had a bottle of port wine. The pulse is now 104, of better strength; lips and teeth covered with dark coloured sordes for the last few days. Feet rather cold; features collapsed and sunken; has been rolling the head upon the pillow a little; rigidity of arms gone; one motion.

The feet to be kept warm. Wine to be continued.

Vespere.—Has had a good deal of crying and vociferation dur-

sponded to elevations in the *dura mater*. These elevations had the appearance of fungous excrescences, which had been lodged in the holes before mentioned.

"The veins of the hemispheres of both sides of the brain were distended with firm clots of blood, which, when the vessels were cut open, seemed mostly solid, but contained also some grumous blood, and at other parts they were filled with solid pale-coloured lymph.

"Under the arachnoid membrane, also, was effused a considerable quantity of serum, of a pale opaque colour, in whitish opaque patches. On the upper surface of the left side the vessels were also distended with clotted blood; and there was the same effusion of serum and lymph in the subarachnoid tissue of that side.

"The ventricles contained from four to five drachms of serous fluid, the greater part being on the right side.

"The parts forming the walls of the ventricles were healthy, except the *septum lucidum*, which was softened and attenuated, but not broken down.

"At the anterior extremity of the right hemisphere, about half an inch from the median fissure, and attached to the arachnoid and *pia mater*, was a small irregular scale of bony matter.

"Some serous fluid was found in the arachnoid membrane, at the base of the brain. The whole of the brain was healthy, and not unusually vascular.

"The whole of the longitudinal sinus, from the anterior to the posterior extremity, was filled to distension with a solid firm clot of blood, moulded to the form of the vessel adhering to it, and in part presenting the appearance of lymph; and in various parts the appearance of solid lymph, containing in its interior a quantity of semifluid bloody-coloured matter, like purulent matter. This coagulated blood also extended into the *torcular Herophili*.

"The Pituitary gland was large, red, and firm, and presented beneath it a cavity, holding a firm, amber-like, semitransparent substance, as large as a small bean, which, when taken out, sank in water.

"The *sella Turcica* was unusually deep and excavated; and the posterior clinoid processes broke off with very slight pressure, and were then found to be very much attenuated.

"*Chest*.—Upon cutting through the cartilage of the second rib, on the left side, a quantity of air rushed out; and it was found when the chest was opened, that the whole of the left lung was collapsed, and a pleuritic exudation was found effused over its lower and posterior part, while a small ash-coloured aperture was visible in the middle of the lower lobe.

"The lung was entirely removed and placed under water; and when air was sent into the windpipe, it was found to escape by a minute aperture. A section was made through this, and it was

found to consist of a small ash-coloured cavity, emitting a gangrenous odour.

" At another part near the lower part of the upper lobe was a similar softened spot, reddish coloured, but not excavated.

" The right lung was pretty sound, except at its inferior lobe, where, near its posterior margin, was a large piece of pneumonic reddish coloured induration, and a small abscess near the surface. A similar patch was observed on the lower part of the upper lobe.

The uterus was unusually small, but its lining membrane appeared perfectly healthy.

In this very interesting case there are several circumstances well worthy of remark.

Judging from the history of the case, there could be little doubt that the patient laboured under epilepsy. Unfortunately I was never able to see her during a fit; but from the account given of the attacks by her friends, and her state immediately after them, I think I am fully warranted in pronouncing them to have been truly epileptic.

The manner in which the fits affected her was certainly peculiar; they were at first but slight, and occurred singly; and it is difficult to explain why, after continuing a year to trouble her, there should have been so long an interval as seven years without a single attack.

The species of epilepsy, to which it appears to me, the case related may with most propriety be referred, is the *epilepsia cerebialis*. The uterus certainly was small; its structure, however, was healthy, and menstruation had gone on regularly till within a year before she came into the hospital; and although at this time the fits increased in frequency, I cannot think that this was induced by the irregularity of the menstrual function. They more probably were connected with the morbid changes which must at that time have been going on within the cranium, and which were themselves most probably the cause of the uterine derangement, which in its turn might account for the hysterical symptoms which she all along manifested, as she frequently burst into tears without any cause, and was often affected with *globus hystericus*.

The opinion that this case belonged to the class *epilepsia cerebialis* and not *uterina*, is strongly supported by the appearances found within the head. I am fully aware that the proximate cause of epilepsy still remains in obscurity, notwithstanding all that has been said and written upon the subject. As, however, various morbid appearances have been found to occur in cases of epilepsy, and as these have been by some observers believed to be sufficient to produce the symptoms of the disease, and several of these lesions having occurred in the case under consideration, a few remarks on them may be proper.

lobes, there was a yellow, solid, friable matter, which might be raised in bits, &c." Numerous other morbid lesions are detailed. But enough has been said to show that there is certainly in many instances some connection between this disease and these morbid structures, and which the appearances found in the case before us tend to confirm.

The disease found in the lungs was, I confess, wholly unlooked for on my part. There had been no symptoms sufficiently urgent to attract much notice to the chest. She had cough, but not more than is often present in ordinary cases of fever. The respiration was hurried at one time and natural at another, and this seems to be sufficiently well accounted for by her hysterical tendency. She had no expectoration, and the existence or non-existence of pain in the chest could not be determined, owing to her state of incoherence. But even had the extent of the morbid process going on in the chest been ascertained before death, I am not aware that anything more could have been done in her state to avert it. Two blisters were applied to the chest without benefit, and stimulants were carefully withheld, till, as a last resource, they became absolutely necessary. The intensity of the head symptoms, and the fever which, from the existence of the eruptions, the slough on the sacrum, the dark sordes on the teeth, and other symptoms, seemed distinctly to have been of the typhoid character, had completely masked the pectoral complaints.

The left lung was much compressed and reduced in size, and adhered closely to the spine. The perforation through which the air had escaped into the cavity of the pleura giving rise to pneumothorax was caused by the rupture of a gangrenous ulcer about the size of a small hazelnut, round which the pulmonary tissue was hard and condensed. In short, it was exactly similar to the gangrenous ulcers described by Dr Craigie in the 148th Number of this Journal, as having occurred in the last two cases related in his paper on Gangrene of the Lungs, the last of which I had an opportunity of seeing. A similar softened spot existed in the upper lobe, but it was not so far advanced, and had not become excavated. All this extent of disease was obscured, and rendered completely latent by the severity of the head symptoms, to which my attention was almost entirely directed.

DUNOON, ARGYLLSHIRE, Nov. 1851.

ART. III.—*Notice of an Instance of MOLLUSCUM CHRONICUM.*

By HENRY NEBEL, M.D., Practitioner of Medicine and Midwifery at Heidelberg. In a Letter to Dr Craigie.

DEAR SIR,—It is very likely that you do not remember me any more; but I have not forgotten your kindness to me, when

in August 1837, introduced to you by Dr W. Thomson, I had the pleasure to walk round with you repeatedly in the Royal Infirmary, where you also favoured me with your Clinical Report of the fever ward. Hence I take the liberty to write you a few lines concerning your interesting account of an instance of *MOLLUSCUM CHRONICUM*, published in the *Edinburgh Medical and Surgical Journal*, January 1851, which I have read with a double interest, as I happened to observe a similar case of this rare disease about ten years ago.

As your article contains, indeed, a monography of this disease, in which all cases are recorded, that ever have come to public notice, I wonder you did not remember the case observed by WILLAN, and described as *MOLLUSCUM PENDULUM* by Dr Bateman, who represented the same in his "*Delineations of Cutaneous Diseases*," plate lx., fig. 3.* Bateman's work appeared in 1817. Your case came under observation in the summer of 1818, and was read by Dr Bateman, as you mention, to the *Medico-Chirurgical Society* in June 1819. The circumstance that neither you nor Dr B. himself said anything of Willan's case of *MOLLUSCUM PENDULUM*, leads me almost to the idea that you take this as a different disease, though the delineation of it shows a remarkable likeness, at least with the two tumours of the palpebrae of James Roy; and though (page 113 of your article) very rightly, as it seems, only two different species of *MOLLUSCUM* have been distinguished, viz., *MOLLUSCUM DIUTURNUM*, and *MOLLUSCUM CONTAGIOSUM*.

Regarding the case which I happened to observe, I have made the following sketch at the time at which it came under my observation.

On the 13th of November 1841 I was called to the wife of Jacob K., at Handschurhsheim, a village two miles from Heidelberg), who had a fit of apoplexy, and was near the end of her pregnancy. Having gone on the field to work, she did not return home, though it was already late. Her husband, alarmed hereby, went thither to see what had become of her, and found her senseless, lying on the ground, where she had probably lain already for several hours. As he could not bring her away, he returned home to fetch a cart, in consequence of which at least another hour elapsed, till at last the patient was brought home and put to bed. By degrees she returned to life, but was unable to speak, and her left side remained paralysed. After a few days she began to stammer a little, and appeared to be pretty well in her right senses. Labour then came on, and the patient was delivered about six weeks before the term, of a healthy boy. Parturition went on naturally, and without particular difficulty. The uterus contracted itself afterwards completely, and no considerable hemorrhage took place. The patient, nevertheless, died

* It is figure 1st in which this is delineated.—D. C.

the following day; not, as it appeared, in consequence of the delivery; but probably this had been accelerated by the approaching death, as we often see that nature, before the death of the mother, expels the child in order to save it.

The *post mortem* examination showed a blood-coagulum of the size of the fist in the right lateral ventricle of the brain, which by this was exceedingly distended. The serum was already absorbed.

The circumstance in this case most interesting to me was a very rare cutaneous disease, MOLLUSCUM, with which the whole body was covered. The wart-like appendices varied in size from that of a small pea to that of a hazelnut. Some few ones were still larger. Most of them had the colour of the skin. In some places they were of a bluish-red colour. No member of the family of the deceased ever had a similar disease. The disease of our patient first began nine years ago, when she was in the family way. At that time, however, the appendices are said to have been very small. After her delivery they had diminished a little in size, and particularly in turgescence; but with the second pregnancy they enlarged and became more numerous. After the delivery they diminished again, but became no more so small as after the first child-bed. In a similar way things went on in the subsequent pregnancies, until the tumours became at last as large and far extended as I saw them. At most parts of the body, particularly on the trunk, they were so dense, that there was scarcely any where a spot of a square inch without such tubercles. At the side of the nipples there hang three or four appendices bigger than the nipple itself, and of so similar a structure and (brown) colour, that they could have easily been taken for as many nipples.

It was striking how much those appendices, so turgid before, relaxed after the delivery.

The examination of those appendices made after the death of the patient, showed that all the constituent parts of the *cutis* had participated in their formation. The larger ones contained a reddish mass of a similar, though somewhat firmer texture, as the skin of corpses with anasarca; though, even by hard pressing, no fluid could be extorted from them.

A piece of the skin from the breast, of the size of a square foot, covered with those appendices, I have taken away at the dissection and given to the Heidelberg Pathological Museum, where it is preserved.

As to the situation of the tubercles in my case, they were most numerous on the trunk, though there was no part of the surface without them. As to their form, they did not show so much variety as in your case; all of them being spherical or spheroidal, and more or less pendulous, the smaller ones similar to those represented as *molluscum contagiosum* by Dr Bateman, Fasciculus xi., plate 61. Many of them exceeded in size those

represented as *molluscum pendulum* by Dr Bateman, plate 61, fig. 3, to which they also presented a likeness in shape.

I have not published my case, because, isolated as it was, I thought it would not be interesting enough; whereas, ranged along with similar ones, it could amplify the knowledge of this disease: and as the Edinburgh Medical and Surgical Journal contains your comprehensive article about this subject, I think a short sketch of it would perhaps not be unqualified to be published in the same Journal. If you are of the same opinion, dear Sir, I leave it to your own judgment to give from my account what you think proper.

Believe me, DEAR SIR, yours very faithfully,

HENRY NEBEL, M.D.

Heidelberg, 10th October 1851.

Postscript to the foregoing Notice, by Dr Craigie.

The instance of MOLLUSKUM CHRONICUM, seen and noticed by Dr Willan, was mentioned in the original Memoir which was sent to the Medico-Chirurgical Society, in the latter part of the year 1818. Dr Bateman, however, in whose hands the paper was placed, removed the passage referring to it, and substituted the sentence, beginning "Dr Bateman, to whom cutaneous nosography," and ending "with descriptive details."

As I felt that I had no title to modify or alter the paper from the form in which it was read to the Medico-Chirurgical Society, in June 1819, this passage, on the responsibility of Dr Bateman, was allowed to stand as he made it. This is the only explanation which I have it in my power to offer, for the omission which has been remarked by my friend, Dr Nebel of Heidelberg.

With regard to any reasons which may have influenced Dr Bateman, either in altering the paragraph in the original memoir, or in giving to the council of the Medico-Chirurgical Society the opinion that it was unnecessary to publish the paper in the Transactions, I never permitted myself to think in any way. I was young in the profession, without experience in matters of the kind, and I was entirely unknown. Dr Bateman was considerably my senior; he was regarded both by myself and the few friends whom I then had, to be a supreme authority in all matters relating to Cutaneous Diseases, and on many other points; and I felt that to whatever opinion he gave, it was at once my duty and my interest to submit. Whether in this I was right or not, it is now of no consequence to inquire.

While I am now explaining the cause of an alleged omission, it may be proper to mention what may be regarded as another omission. In May 1841 Mr Frederick Hall Thomson published in the Lancet a notice of a case, which he refers to the head of MOLLUSKUM CHRONICUM. In this instance the skin presented

many tumours or spherical bodies varying in size; and on the thigh was a tumour which Mr Thomson regards as medullary. It is not perfectly clear to me that this is an example of genuine *MOLLUSKUM*; but as I had no means of knowing any thing about it except from the printed account, I thought that it was most proper to say nothing of its nature. Though the diagnosis of *MOLLUSKUM* is not in general difficult, yet it appears to me that to this head have been referred several examples of disease, which either doubtfully belong to it, or are altogether different. In my former paper I found it necessary to say regarding one of the cases given by M. Jacobovics, that it did not appear to belong to the same head as the *MOLLUSKUM CHRONICUM*, but was either a different, perhaps anomalous eruption, or was the effect of mercurial irritation. Another case which he has given is evidently an instance of tubercular cancer of the skin.*

That these remarks upon the necessity of caution and discrimination, in referring to the head of *MOLLUSKUM*, cases of tubercular disease of the skin are not uncalled for, will, I think, be rendered manifest by the following facts.

In the last published Number of the American Journal of the Medical Sciences, Dr Henry H. Smith of Pennsylvania records under the title of *MOLLUSKUM* developed by injury, an instance in which the skin of the left arm in a married female, aged fifty-five years, became covered in consequence, it is stated, of injury, by numerous small bodies or tumours, first about the size of peas, afterwards a little larger, and finally like small shot or bullets. The mass was first removed by the escharotic effects of the chloride of zink. The tumours returned in the cicatrix; and the arm was removed by amputation. Afterwards the stump and arm were attacked by them; and eventually the skin of the head, trunk, and other parts became occupied by these bodies. Dr Smith afterwards allows that this is medullary sarkoma; and it is difficult to see to what other morbid formation it can be referred. It seems, therefore, not accordant with correct diagnosis, or correct pathological principles, to apply to this disease the name of *MOLLUSKUM*. Yet such is the designation of this case. If it teaches nothing else, it shows the confused and inaccurate notions which prevail upon this subject.†

In making these observations, nevertheless, I wish it to be understood, that though I am desirous to fix upon just and irrefragable grounds, the character of the *CHRONIC MOLLUSKUM*, and avoid the error of referring to it every unknown and anomalous appearance of tubercles on the skin, I am perfectly willing to

* Edinburgh Medical and Surgical Journal, Volume lxxv., page 119.

† Case of Molluskum developed by an Injury, and presenting under the Microscope the Character of Medullary Cancer. By Henry H. Smith, M.D., Assistant Lecturer on Demonstrative Surgery in the University of Pennsylvania. American Journal of the Medical Sciences. No. xlv. New Series. October 1851. Article vii., p. 396.

allow to others who have witnessed these cases and carefully examined them, the responsibility of classing them under the head of *MOLLUSKUM*, if they see cause sufficient and satisfactory for such course.

I may observe, however, that so far as correct observation hitherto goes, it has not been proved, either that the *CHRONIC MOLLUSKUM* is connected with Medullary Carcinoma or Tubercular Cancer of the skin, or partakes in any way of those tubercular eruptions which occasionally in strumous and bad habits of body succeed to the use of mercury.

ART. IV.—*Analytical Inquiries on the Physiology and Pathology of the Spleno-Hepatic Apparatus.* By Dr J. H. S. BEAU,* Physician to the Hospital Saint-Antony, Assistant to the Medical Faculty of Paris. (*Archives Generales de Medecine*, January 1851. Quatrieme Serie, Tome xxv.)

UNDER the name of Spleno-hepatic Apparatus, I include the Spleen, the Liver, and the Portal Vein, which forms the communication between these two organs, and which places them in relation with the infra-diaphragmatic portion of the Digestive Canal.

I shall consider the Liver, the Portal Vein, and the Spleen, first in the Physiological or normal state, and afterwards in the Pathological or diseased state.

PHYSIOLOGICAL PART.

In order to study usefully the functions of the Liver, the Portal Vein, and the Spleen, it is indispensable to examine the subject in a historical manner. I shall, therefore, state the principal opinions that have prevailed among physicians, beginning with Galen, the first who treated this physiological question with the importance which it deserves.

The following passage from Galen shows what was the opinion of the ancients and of himself on the subject of the *Vena Portæ*. “But as to these veins (the Mesaraic), as servants in cities, convey into the common store-house of the city the corn cleansed in the barn, to be prepared, and immediately to be rendered fit for the purpose of nourishment, in like manner, the food previously elaborated in the stomach, these veins convey to a certain place

* At the meeting of the Academy of Medicine on the 14th of March 1850, M. Beau read to the Academy a Memoir bearing the title of *Pathogenic Considerations on the Liver*. This Memoir contained some parts of the present, among others, the history of the functions of the liver, and the mode in which the *ingesta* act in the production of hepatic disorders. The principal ideas of the present Memoir have been deposited at the Institute in 1846.

of digestion common to the whole animal body, which we name the liver. But the entrance into this place, divided into many narrow paths, is one which some ancient person, skilled, I believe, in nature, called ΠΟΡΤÆ (Πυλαι). This name has remained from that time to the present day. This name, further, Hippocrates himself, and, along with him, the whole family of the Asclepiadæ, adopted, praising the wisdom of that ancient philosopher, who compared to the civil distribution that which takes place in the animal body.*

Thus Galen compares the mesaraic veins to the porters and carriers, who, in a well-governed city, convey the corn, previously cleansed, into a common store-house, in order that thence it may be distributed for the food of the inhabitants.

In the same manner, these veins convey the alimentary matter which has been prepared in the stomach, into a place of second elaboration, which is the liver. Among all the narrow entrances of this organ, the *Porta* he regards as the one in which all these are united.

In the following paragraph from the chapter entitled *de Usu Hepatis*, Galen returns to this action of the liver and the Mesaraic veins; but he adds a new circumstance, namely, that alimentary articles before being subjected to the action of the liver, have undergone in the mesaraic veins a previous modification, by which they are coarsely assimilated to the nature of the blood into which they are conveyed. "The liver itself," he observes, "after it has received from its servants that nutriment in a prepared state, and presenting a rude and obscure resemblance to the blood, effects in it the final preparation to the formation of perfect blood."† Thus according to Galen the liver effects in the alimentary matters those final changes which fit them to assume the characters and perform the duty of blood.

Again according to Galen, the liver cannot convert the aliments into fluid blood, without eliminating certain impure and excrementitious matters which are found in alimentary substances. These matters are of two sorts; one light and yellow is received by the gall bladder; the other thick and turbid is conveyed into the spleen.‡ According to this view the gall bladder is the receptacle of the yellow bile; the spleen is the spongy receptacle of black bile, (*atra bilis*), or *melancholia*; and by this spongy organization, the spleen ought like a sponge to absorb the melancholic fluids of the liver.§

* De usu Partium, p. 135. Apud Juntas, 1565.

† Ipsum autem hepar, postquam id nutrimentum acceperit a famulis jam præparatum et veluti rudem, quandam delineationem obscuramque speciem sanguinis referens, inducit ei postremum ornatum ad sanguinis exacti generationem.

‡ Siquidem vesicam quæ leve et flavum superfluum receptura erat, natura imposuit hepati; splenem vero qui crassum et limosum attracturus erat. Ibid. p. 136. B.

§ Facultatem habet splen ad seipsum trahendi succos melancholicos, rarum ad modum ac laxum instar spongiæ cujusdam, ut succos crassos facile et attrahat et excipiat. Ibid. p. 140. B. De Lienis Usu.

he pretended to possess as to a method of embalming dead bodies. De Bils published in 1659, at Rotterdam, a Dissertation, in which he maintained that the chyle passes in great part into the liver by means of the *vena portæ*, and that this gland is the organ which prepares the blood.*

The publication of this performance raised a universal opposition. "Against this heresy," says Haller, "almost the whole anatomical world arose in resistance."† Bartholin published a refutation of the experiments of De Bils, in the title-page of which, taking it for granted that the liver was scientifically dead, he represents the resurrection of this organ to be a matter of great difficulty.

Thus terminated this controversy. The honour remained with Bartholin; that is to say, the liver was decidedly interred, as this anatomist wished. It ceased to be regarded as a sanguifying organ, as an organ concerned in the function of Haematosiis. This function was progressively assigned to the lung, which, in consequence of the beautiful discovery of Harvey, was considered for the first time, as an organ entrusted with the function of preparing and modifying in a powerful manner the circulating fluid.

Changes also took place as to the bile; but this fluid acquired increased importance. It was no longer regarded as an excrementitious fluid, but as a liquid separated from the blood by the glandular and secreting action of the liver for important purposes, and as one indispensable to digestion.

The organ which suffered still more than the liver by this revolution in physiological doctrine, arising from the discovery of the chyloferous vessels, was the *Vena Portæ*. Bartholin had indeed more reason to write the epitaph of the portal vein than that of the liver; for while the liver continued to exist as the organ which secretes the bile, the portal vein, deprived of its functions of absorption, no longer enjoyed any special value. Its terminal rootlets ramified on the one side in the alimentary canal, on the other in the substance of the liver, remained as a fact inexplicable and singularly exceptional in viewing the anatomy and physiology of the venous circulation.

Such were then the results of the revolution which took place in physiological science by the discovery of the lymphatic vessels;—the liver dispossessed of its functions of hæmatosis, and concerned only in the secretion of bile; the absorption of alimentary substances effected by the chyloferous vessels to the exclusion of the *vena portæ*. These are the physiological notions which we find on this subject in all the authors of the last and the beginning of the present century;—in the works of Haller, Hunter, Bichat, and similar writers.

* Bilsii Epistolica Dissertatio, qua verus hepatis circa, chylum et pariter ductus chyli ferri hæctenus dicti usus docetur. Rotterdami, 1659.

† Haller Bibliotheca Anatomica Bilsii. Bartholini Responsio de experimentis Bilsianis et difficili hepatis resurrectione. Hafniæ, 1661.

important office of assimilating branches, that is, they are conveying virgin blood, which is to add its powerful action to that of the blood of the absorbing branches, in order to aid in the assimilation of the matters absorbed.

We have now proceeded on the supposition that the absorption of alimentary matters is accomplished only by one single point of the gastro-intestinal tube; but we know that this is not always the case. Often indeed after the introduction of considerable quantities of food, the absorption of the assimilable matters is effected at a certain time of the process of digestion, by almost the whole system of the veins of the digestive canal, and consequently at that time the gastro-intestinal venous branches almost all perform the office of absorbing branches.

Now the blood which arrives from the stomach and intestine, and which thus transports the matters absorbed, being too inconsiderable, in proportion to the mass of these matters, would be in like reason, as has been already said, powerless to effect in them suitable assimilation. But for this deficiency nature has made provision, by utilising for the necessities of this assimilation the veins of the mesentery, of the omentum, of the pancreas, of the gall-bladder, and of the spleen, which convey by numerous affluent branches a large supply of assimilating blood upon the matters absorbed by the gastro-enteric veins.

Matters being understood in this manner, there is no longer anything incongruous in the functions apparently different of the original or convergent branches of the portal vein, of which some, namely, those of the digestive canal absorb, while others, namely those of the pancreas, the epiploons, the spleen, and similar organs do not absorb. Of all taken collectively, the object and purpose is to absorb and assimilate the alimentary matters. The first class of veins are at the same time absorbent and assimilant, while those of the second class are purely assimilant.

It is further necessary that there should be branches purely assimilant, because it is necessary that, how considerable soever be the quantity of matters absorbed, there should at all times be in the system of the Portal Vein a mass of blood still greater in amount, in order to transport and assimilate these matters. In order to meet this necessity, Nature doubtless wished that the veins of the mesentery, of the epiploons, of the pancreas, of the gall-bladder, and of the spleen, should contribute their contingent of dark coloured blood to the current of the Portal Vein, instead of pouring it into that of the *Vena Cava*.

It further results from the foregoing considerations, that before the formation of the trunk of the Portal Vein, the composition of the blood is homogeneous, because in certain branches it conveys alimentary matters, and in other branches it contains none at all.

unfit for hepatic sanguification, and fulfilling nevertheless important functions in the process of digestion.

3d. Everything leads us to admit, with Galen, that the substances ingested and absorbed before arriving at the liver undergo a primary modification or assimilation from the blood of the Portal Vein. We have quoted one passage from this author on this subject. Here is a second passage in which the same opinion is stated in very positive terms; "Venis quæ ad ventriculum et universa intestina pertinent, inest facultas quædam sanguinis effectrix, qua succum qui ex cibis distribuitur, venis in sanguinem mutare naturale est, prius quam is ad hepar perveniat."*

There is in the theory of Galen one point upon which modern physiologists are completely at variance with this author. It is that which relates to the origin and nature of the chyle.

It is the opinion of Galen that the entire nutritive part of the aliments is reduced in consequence of elaboration in the stomach to one single principle, always identical, a true succus or juice, as may be seen in the preceding passage. To this Galen applies the Greek denomination of chyle (Χυλος), which means merely *Succus*. It is this fluid or *succus* which is conveyed by the Portal Vein to the liver; and in some passages Galen calls this last vessel the *Vein of the Chyle*.

This error of Galen upon the unity of the alimentary juice or chyle has prevailed among physiologists to the commencement of the present century. It was the cause of the violent discussions which took place on the subject of the liver at the period of the discovery of the lymphatic vessels. It was indeed very difficult, or rather impossible at that period to arrange the liver along with the chyliferous vessels; for from the moment that it was well established, that the alleged single alimentary principle, the chyle in short, traversed the chyliferous vessels in the form of a white milky fluid, in order to proceed by the thoracic duct into the subclavian vein, physiological reasoners must have inferred, that the nutritive or chylous portion did not penetrate into the blood by the Portal Vein and the Liver; and, consequently, they must have refused to ascribe to the liver the functions of Hæmatosis, which Galen had assigned to that organ.

By means, however, of the recent discoveries, and the popularized form in which they have been taught by M. Berard, it is possible to conciliate the adversaries of the preceding discussions; for upon the true nature of chyle, physiological opinion is perfectly fixed. It is known that the liquid which bears this name is nothing else than the fatty portion of the alimentary matters converted into an emulsion by the pancreatic liquid. All the other alimentary substances, drinks, and similar articles, are absorbed by

* Galeni Opera De usu Partium ; De usu Hepatis. 138 E.

the same name, and it furnishes almost the half of the whole quantity of blood which traverses the trunk of the Portal Vein.

There is in the veins of the stomach a peculiar disposition which to me appears to indicate the assimilating function of the splenic venous blood. This is the arrangement of the veins of the large *Cul de Sac*, which hardly detached from the part of the stomach by which they are sent off, proceed by the shortest and most direct line under the name of *Vasa Breviora* towards the splenic vein with which they communicate. These *Vasa Breviora* of the great *Cul de Sac* of the stomach, it appears to me that we ought to understand, in the following manner. We ought to admit at first that these veins, by reason of the portion of the stomach from which they issue, absorb during the process of gastric digestion a large quantity of the ingested substances. These substances which we suppose may be very active, being introduced into a current of blood, proportionally not very abundant, would have been badly assimilated by it, or even might have been capable of modifying or altering it, if they had been placed in the most speedy way possible in contact with a column of blood, virgin or new, for all reaction. It is probably with this intention that nature, instead of making the veins of the large *cul de sac* follow the whole length of the large arch of the stomach, before effecting their communication with the Portal Vein, has caused them to open by the shortest and most direct line into the splenic vein.

We have found in the nature of the Portal blood a proof of its assimilating property; we have in the like nature of the splenic blood the same proof of the same property. This resemblance in nature, the analyses of M. Beclard place beyond all doubt.

"Extracted by beating," says this observer, "the fibrine of the splenic blood is decolorized with much greater difficulty than the fibrine of arterial or venous blood; it is less elastic; it does not separate into ropy filaments, but into small masses which are agglutinated to each other. When left in contact with air, the splenic fibrine is at the end of twelve hours almost entirely liquified. The fibrine extracted at the same time from the blood of the jugular vein, and placed in the same atmospherical conditions, instead of being liquified, parts on the contrary with its water and is dried."

The blood of the splenic vein therefore is characterized with that of the portal vein, by containing a deliquescent fibrine; blood which consequently possesses little coagulating power, and is eminently well adapted to exert a solvent and assimilating action. This fact of chemical analysis completes the proof, as has been above said, in the estimation of MM. Bernard and Mialhe, that it is not necessary to consider as *albuminose* all soft fibrine found in the

blood of the portal vein; for the reason that the splenic vein which does not issue from the digestive tube, and which accordingly cannot convey from that apparatus albuminose, furnishes, nevertheless, soft fibrine as well as the mesenteric vein.

This speedy transformation, in the interior of the spleen, of arterial blood into splenic venous blood, supposes a physiological operation analogous to that which takes place in glands. Now in point of fact this transformation is thus accomplished, since the spleen, as has been shown, is a glandular organ.

Physiologists, nevertheless, may not wish to allow that the simple transformation of red blood into modena blood, supposes a glandular action; and on this point they may maintain with Haller, the principle that there is no gland where a particular liquid is not secreted.

Upon this point we remark, that in the spleen there is not a simple transformation of arterial blood into ordinary venous blood. The splenic venous blood is a fluid of a peculiar nature. This is a fact long admitted by all physiologists and by Haller himself, though it was not precisely known that the character of the splenic venous blood is that it possesses a fibrine which is liquefied in the air, instead of being dried. It may therefore be asked what is to prevent us from thinking that this special blood is a species of ordinary venous blood, the fibrine of which is thus modified by a liquid secreted by the glandular grains of the spleen. And as it would be without purpose, that such a liquid should be secreted separately into particular ducts, nature mixes it with the venous blood as speedily as possible; that is to say, while it is in the state of forming particles, and thus causes it to flow in the same vessels.

Whatever may be thought of this hypothesis, which I offer merely as a means of enabling readers to comprehend the special nature of the splenic venous blood in its relations with the glandular structure, I think the question may be stated whether it is wise to say with Haller, "That no organ can be considered to be a gland which does not furnish a secreted liquid." These principles, laid down by scientific authority, to give force to certain facts which are thought to be well known, have, in my opinion, the inconvenience of desiring to render the future fixed, and accordingly to stop progress. Who knows, indeed, if, beside the glands properly so called, which secrete a particular liquid, there are not other sorts of glands, as the spleen, the function of which is to produce a species of venous blood, quite different from ordinary venous blood in its nature and uses.* We may mention, in passing one

* We ought to refer to this species of gland, the thyroid body, and the thymus gland, the function of which would be to secrete a peculiar species of venous blood, and to pour it into the current of the *vena cava*, in order to modify the blood, and to prepare it probably for pulmonary hæmatoris.

no longer be sensibly different from that of the ordinary venous blood.*

I have stated all the reasons which tend to establish the inference of the assimilating functions of the spleen, and which accordingly indicate this to be a very important associate to the gastro-intestinal veins.

We must, nevertheless, give a little attention to a theory of the functions of the spleen, which has not yet been under consideration. I refer to the theory of Tiedemann and Gmelin.

These physiologists are of opinion that the spleen serves to secrete a red coagulable lymph, which it transmits by its lymphatic vessels to the thoracic duct, in which it is to contribute to the assimilation and to the change of the ordinary characters of chyle into blood.† The mesenteric ganglions also act in the same manner, but in a degree less energetic. In other points, the reasoning upon which they found their theory is the following.

“We made the same remark upon the chyle which we collected from the lymphatic vessels of the intestinal tube in horses, before the entrance of these vessels into the mesenteric glands. This chyle was always white, and never reddish, and did not undergo coagulation; or, if it was coagulated, it was always slowly and imperfectly; whereas, the chyle, when it proceeded from the lymphatic vessels, which issue from the mesenteric ganglions, had a reddish colour, and it then became coagulated more easily and more completely. *Lastly*, it appeared much redder still in the thoracic duct, after the introduction of the red coagulable lymph which the lymphatic vessels from the spleen brought to this tube, and there, also, it underwent more prompt coagulation. From these observations, it accordingly results, that the chyle is purified by successive degrees by its passage in the mesenteric glands, and that by its mixture with the red coagulable lymph of the lymphatic vessels of the spleen, it approaches the blood in quality, and becomes more and more like this fluid.”‡

From the considerations into which we have hitherto entered, we ought quite naturally to be led to admit this theory of Messrs Tiedemann and Gmelin, connecting it, however, with that which we have previously unfolded, in such manner, that, meanwhile, the spleen must appear to be an important organ, charged with a

* M. Beclard proceeding on this inequality in the proportion of the globules presented in the splenic blood and the mesenteric blood, admits that the globules are destroyed in the spleen, and are formed in the mesenteric vein. It is in other points to him a simple opinion, surprising in the present state of science, and thus he thinks he can make it be accepted only when he shall have shown the laws which regulate the movements of the Portal Blood.

† Die Verdauung Nach Versuche. Heidelberg, 1826.

‡ Die Verdauung Nach Versuche. Heidelberg, 1826. Page 92.

two-fold assimilation. On the one hand, by the venous blood which it furnishes, it modifies the alimentary substances which have been introduced into the Portal system, and disposes them to undergo the elaboration of hepatic sanguification. On the other hand, by the peculiar lymph which it transmits to the thoracic duct, it equally modifies the chyle, that is the fatty portion of the alimentary articles, and causes them to approach to the nature of blood before this change is completed in the respiratory function. In this double assimilation, however, venous and lymphatic, it is to be remarked that the spleen does not act alone; this organ receives powerful assistance on the side of the Portal system, from the blood of the other venous branches besides the splenic vein, and on the side of the chyliferous vessels from the lymphatic glands of the mesentery. This point shall again come under consideration.

I have not yet quoted a theory of the functions of the spleen, lately promulgated, and sufficiently allied to that which I here propose. M. Berard is of opinion, that the mass of blood which issues from the spleen goes to be added to that which arrives loaded with alimentary substances from the gastro-enteric veins, and this with the single purpose of diminishing the relative proportion of heterogeneous materials which this blood conveys in its stream.* There ought to be in the splenic blood more than this function of simple mingling; for if it is true that the blood possesses the property of modifying and destroying certain substances which have been ingested, everything leads us to infer that this property resides also in the blood, which is placed first in contact with the aliments, that is to say, the blood of the Portal vein and of the spleen. From this assimilating influence of the blood, however, we must except those substances which are not susceptible of assimilation; for instance, mannite (Mialhe), or energetic substances taken in large quantity, as alkohol in the case of intoxication.

Hitherto we have studied in the spleen only the function which it performs by the aid of its two fluids, venous and lymph. But, as is known, the spleen is not only a gland; it is a gland dilatable and retractile. It is requisite then for us to show what uses are fulfilled by the vasculo-areolar element, to which, as well as to its investing membrane, it owes the remarkable faculty of being capable of being dilated and again contracting itself.

For this purpose, I cannot do better than cite an experiment well known, simple, and easily repeated.

After having exposed in a living animal the trunk of the Portal Vein, we compress it between the fingers, or a pair of forceps, in such a manner as to interrupt the continuous movement of the

* *Cours de Physiologie.* Par M. Berard. Tome ii., p. 552.

venous current, which is proceeding toward the liver. We observe then a swelling of the Portal Vein, and of its branches above the point compressed, which swelling is proportional to the degree of constriction employed; but we observe further a very apparent swelling of the spleen, which may be greatly augmented in volume. When we cease to compress the Portal Vein, the dilated branches are emptied at the same time as the spleen, which resumes all at once its original size, expelling by one single jet the blood by which it was distended.

It is easy to understand what takes place in this experiment. I shall, nevertheless, state its successive stages, the details of which are important.

We have first to notice the obstacle which the venous current encounters at the point of compression. The blood stopped and obstructed at this point increases in its mass, by the total quantity which necessarily is incessantly flowing by the original or convergent branches of the Portal Vein. Hence the dilatation of the veins placed above the point of compression; hence the dilatation of the spleen which is distensible like the veins, by reason of its vasculo-areolar network, and which further receives from the splenic artery a large quantity of blood, which the Portal Vein obstructed by compression can no longer receive.

From this dilatation results both to the veins and to the spleen, tension of the *parietes* with tendency to contraction. This contraction which is elastic in the veins, appears to me to be contractile in the spleen, particularly if we are guided by recent experiments.* By this property of contraction, the veins and especially the spleen, expel by a single jet the excess of blood by which they are distended, and immediately recover their original volume.

This impediment causes dilatation; dilatation induces tension, with tendency to contract; and tension becomes a force with which the veins, and especially the spleen, contend against the obstacle which opposes the current of the Portal Vein.

The obstacle now under consideration depends, as has been said, on compression of the trunk of the Portal Vein produced by the fingers or the forceps. There are, however, other obstacles quite as real, though not acting in the same manner, which induce the same results.

Thus, whenever respiration is suspended, as in making great efforts running, during the tetanic rigidity and similar states, and when the large venous trunks are not emptied by inspiration,† the inferior

* The Society of Biology has recently proved, that we may produce a remarkable degree of contraction in the spleen, by subjecting it in a living animal to the contact of the electro-magnetic apparatus.—*Memoires de Societe de Biologie*. T. L. p. 157.

† See, on this subject, an interesting Memoir by M. Berard, entitled, Effects of the Elasticity of the Lung.—*Archives Generales de Medecine*. Juin 1830.

the disposition of the vessels in the liver shows, that Nature has taken precautions to obviate the dangers arising from this excess of blood in the gland. Two considerations deserve notice.

1st, We may observe the presence of the Capsule of Glisson around the trunk of the Portal Vein and its hepatic branches. Of this capsule, which thus doubles the wall of the Portal Vein towards one point, where it requires to support the whole effect of the reaction of its convergent and abdominal branches and that of the spleen, the evident object is to limit its movement of expansion, in the instances of superabundance, in order to obviate any hurtful compression of the portion of the hepatic parenchyma, which is contiguous to the trunk of the vein and its terminal branches.

2d, Means of derivation are provided for diminishing the excess of blood when it is threatening to be injurious. Such are the small vessels recently discovered by M. Bernard. These vessels establish a direct communication between the rootlets of the Portal Vein and the part of the *vena cava* which is adherent to the liver. Thence they allow to escape the excessive fulness of the Portal Vein, and pour this excess of blood into the *vena cava*, without causing it to pass through the sub-hepatic veins. M. Bernard has established as a fact the existence of these vessels, especially in the horse. The use of this arrangement is understood without difficulty, when we reflect on the domestic habits of this animal, which is frequently obliged to run immediately after a copious meal, and which, consequently, is thus placed in the condition most proper to induce a considerable degree of fulness in the Portal System.

We return to the subject of distensions of the spleen.

From the foregoing analytical exposition, we draw the following conclusions as to the occurrence, manner, causes, and purposes, of distensions of the spleen.

1st, When there is an obstruction to the progressive motion of the blood in the Portal System, the spleen is less distended by the entrance of the blood, which returns to it by a retrograde course, than by the afflux of that which is brought to it in abundance by the splenic artery.

2d, The spleen is then converted into a reservoir with tense, yet contractile walls, with a power of reaction proportioned to the quantity of blood accumulated, and able thus to contend against the obstacle which retards the normal progressive movement of the blood.

3d, This contractile impelling agent was particularly necessary in those cases in which a proportion, often considerable, of alimentary substances being added to the blood of the *vena portæ*, produces a mass of fluid difficult to be moved.

of symptoms was not an imaginary result of preconceived ideas in Dr Dobson, is proved by the fact, that the dogs which he fed in this manner, by means of large meals given at remote intervals, pined away gradually, and died at the end of one month.*

All these symptoms of fulness, the pining away and the death of the animals, Dr Dobson explains, by referring them to his theory, which is, that the spleen is a *diverticulum* or reservoir intended to receive the excess of blood, which flows to the stomach during the entrance of food into the blood-vessels. I would say, that the symptoms of plethora and fulness depend on this fact, that there is in the Portal System at this time a mass of blood loaded with an excess of alimentary substances, which, failing the continued impulse of the spleen, encountered great difficulty in getting through the capillaries of the liver, and consequently distended the Portal Vein.† As to the mortal pining, which terminated these copious meals at the end of one month, the explanation of it is to be given by reflecting on the great excess of alimentary substances which no longer found either in the Portal Vein or in the lymphatics a sufficient amount of assimilating liquid. The assimilation was accordingly imperfect; the elaboration by the liver was badly performed; and, consequently, nutrition was defective. Hence the wasting and death.

As may be well imagined, it would be particularly important to observe in the human race the phenomena which show their presence after the extraction of the spleen; for we might have, in certain abnormal sensations, information which is completely wanting, when we reason from experiments performed on animals. The instances, however, of removal of the spleen in the human race are extremely rare. I know only two authentic examples, that is to say, cases in which the fact of the absence of the spleen was ascertained by inspection after death.

The first case is related by Fantoni. This was observed in the person of a female, who, after extirpation of the spleen, enjoyed a degree of health sufficiently good to become pregnant.

* London Medical and Surgical Journal, 1830, and Archives Generales de Medecine, Nov. 1830.

† Among the symptoms of fulness enumerated above, and observed by Mr Dobson, the frequent excretion of urine deserves particular attention. This phenomenon has a peculiar value in reference to the ideas of M. Bernard, who admits that, in the instances of fulness of the Portal System, the excess which may have entered the *vena cava* regurgitates by that vessel as far as the kidneys, in which it is separated from the blood in the shape of urine.

The symptoms of fulness before-mentioned would have been much more considerable, if the dog, deprived of the spleen, had been obliged to run after taking his copious meals. The spleen does not restrain running, as has been said; on the contrary, it favours that act, by accelerating the movement of the blood in the Portal Vein.

great influence both of different kinds of food in maintaining and deranging health, and, above all, to the action of stimulating drinks, both directly upon the gastro-enteric tissues, and indirectly upon the tissues of the veins, the blood flowing within these veins and the structure of the organs, especially the liver, through which these substances are conveyed and distributed by the blood. The absorption of various articles ingested, especially in the liquid form, by these veins, is a fact which has been long familiar to the minds of physiologists. But the systematic and extended application of this doctrine, in the manner in which it has been done by M. Beau, may be useful in directing the attention of physicians to the powerful influence of these veins, both for good and evil, in the animal economy. The latter part of the doctrine is more fully elucidated in the second part of the Memoir.

One part of the doctrine of M. Beau may not be so readily received as that to which reference has been already made. It is that which relates to the assimilating power which he ascribes to the blood of the Portal Vein. In the usual doctrine the assimilating power, such as it was, was ascribed to the venous blood of the superior cava. But this was supposed not to be considerable, or rather to be non-existent, until the blood was subjected to the aerating action of the lungs, to which organs accordingly the assimilating function was principally ascribed. According to the doctrine of M. Beau this process begins a step earlier; and, though it may receive its final completion in the blood-vessels of the lungs, it is commenced, according to him, and in a great degree performed in the vessels of the Liver. A question naturally arises;—What are the qualities of blood which enable this fluid to exert assimilating powers?]

ART. V.—*On Some Points connected with the British Naval-Medical Department and that of the United States.* By THOMAS STRATTON, M.D. Edin.; Surgeon Royal Navy.

It is interesting to compare the arrangements in one service with those in another, and with this view I beg to offer to the readers of this extensively-circulated Journal a short notice of the medical department of the United States' Navy.

On the first of January, 1851, there were in the United States' Navy 150 medical officers, of whom 69 were surgeons (including fleet-surgeons), and 81 assistant-surgeons, of whom 38 were passed-assistant-surgeons.

The pay of these officers in their several ranks and durations of service is exhibited in the following table.

are allowed forage. The Surgeon-General has 2500 dollars a-year, not including allowances; his total monthly pay, including allowances, is 208 dollars, 33 cents, or £168, 6s. sterling, for a month of thirty days, or £5, 12s. 2d. a-day, being £2047, 12s. a-year.

[It may be observed here, that the medical Director-General of the British Army lately retired on his full-pay of £2000 a-year; less than the above, although from the one army being so much larger than the other, the amount of duty must have been much greater.]

The *total monthly pay*, that is, including allowances, of surgeons of ten years' service, is 149 dollars, or about 20s. 8d. sterling a-day.

Surgeons under ten years, receive 125 dollars, or about 17s. 4d. sterling a-day.

Assistant-surgeons, of ten years' service, receive 122 dollars, 50 cents a-month, or about 17s. 2d. sterling a-day.

Assistant-surgeons of five years' service, receive 98 dollars, 50 cents a-month, or about 13s. 9d. sterling a-day.

Assistant-surgeons, under five years' service, receive 81 dollars and 83 cents per month, of thirty days, which is equal to about 11s. 3d. sterling a-day.

I am afraid that some readers will consider that these details are somewhat misplaced in a Journal so scientific as the *Edinburgh Medical and Surgical*; if such there be, to them I offer as an apology the reflection that the successful cultivation of science depends a good deal on the degree in which it is rewarded in a pecuniary as well as in an honorary* manner.

I trust that in a communication where so many figures occur, and which will not be corrected by the writer, the printer will be careful not to make any errors for the candid reader to pardon.

PRINCE-EDWARD ISLAND, }
October, 1851.

* See on this subject the eloquently written pamphlet entitled *Summary of the Claims of Medical Officers of the Army and Navy to Military Rewards and Distinctions*. London: Churchill. 1849. Pp. 24. Reprinted from *Lancet* of 22d September.

there must have been great compression, as well as swelling, to force it up between the two.

" These questions of strangulation, I believe, are very difficult ones?—They are difficult, but not so difficult if we know the exact state of the body and cord; for then, I think, they may be distinguished. Suicidal might be easily mistaken for homicidal strangulation; homicidal strangulation cannot be easily mistaken for suicidal, because it is scarcely possible to place the body, or the cord round the neck in such a way as to deceive those who have attended to these subjects.

" In a case of decided homicidal strangulation, is it not easy to simulate suicide?—No, because in such cases too little or too much is done; in this case there was too much done. There may be cases in which it is difficult to distinguish between them, and there are others in which there is no difficulty; every case must be judged of by its own circumstances.

" In your book I find you refer to a celebrated case, which was pronounced by some to be suicidal and by others homicidal; there was a handkerchief tied round the neck so tightly that it was difficult to remove it. [He read the case.] I believe that gave rise to considerable discussion, and you in your book allude to it?—Yes: but in that case there was a knot to keep the handkerchief fixed, and the persons to whom it was referred formed an opinion that it was a case of suicide from the moral circumstances.

" In another case stated in your book, a woman was found lying on her stomach in her bed, and a woollen garter tied twice round her neck with two knots, and the circumstances of that case, you state, left no doubt that it was a case of suicidal strangulation?—No doubt of it; but the circumstances were widely different from those observed in this case.

" Would not the first knot of the ligature round her neck have had the effect you speak of in this case of depriving her of consciousness and power?—No, because the first knot, it appears, was put on the throat above the larynx, and the second knot was over the first; I imagine that the first constriction was not very tight, and was on that part of the neck where it allowed a certain degree of respiration.

" But it caused death?—It was sufficient to cause death, although probably not immediately, because death may be produced by any partial obstruction of respiration, as by a stocking tied round the throat.

" I will call your attention to another case here, that of a lunatic. The patient was found with his head hanging over the side of the bed; and when it was attempted to raise the body, it was found that respiration had ceased, and that the neck was con-

ceal it when perpetrated, under the pretence of suicide. The mode in which the cord was found lying near the left hand of the deceased also tallied with the view that a rude attempt had been made by the party who had perpetrated this crime to simulate the act of suicide.

The *general* and *medical* evidence further fixed the crime on the prisoner, by the fact that an appointment had been made by him to meet the deceased on the evening of her death; that his time was unaccounted for during the period which would have corresponded to the appointment and the time of her death; that, when taken, there were marks upon his clothing, proved by microscopical and chemical examination to be mammalian blood, while he referred the stains to a vegetable substance which could not possibly have produced them. These stains were fresh and still moist when he was first arrested; and an attempt had been made to remove, by wiping or rubbing, those which were more prominent, and in the front of his dress. Finally, the deceased had not been robbed or ravished; her body was found lying in a lonely spot in a field out of the high road, not far distant from the place of appointment made by the prisoner; and there was no reason for supposing that the hand of any stranger, or of any other person but the prisoner, was concerned in her death. The only marks of violence on her body were those which would have arisen from the mode of death, and the struggle for life during the act of strangulation. She had bled in *two* different places, her face was flattened by great pressure, and her bonnet crushed; the upper part of her dress was torn, while her lower garments were arranged in a smooth and orderly manner. All of these circumstances corroborated the medical inference, from the state of the cord round the neck, that this could *not* have been an act of suicide. It was therefore clear that either the prisoner, or some other person, must have committed the crime.

In cases of this kind, questions of time are of vital importance. For reasons to be hereafter stated, it was inferred, that, considering the state in which the body was found, the death of the deceased might have taken place soon after she left her house to meet the accused, or at a later period of the night, *i. e.*, from about seven o'clock in the evening up to half-past ten or eleven o'clock. The evidence for the prosecution brought out the following facts regarding the prisoner:—When asked by the mother, on the morning of the discovery of the body, where the deceased was, he said,—“I don't know, I have not seen her.” The mother said:—“You did see her yesterday, between four and five o'clock, and talk to her, and appointed her to meet you at the stile near my house at half-past six; she went out to meet you, and where

lence than might have been committed by a person determined on suicide."

From what has been already stated, it will be perceived that the defence rested upon two points:—1st, the *possibility* of this having been an act of suicide; and, 2dly, if not, that the prisoner could not have perpetrated it, because the time and circumstances, as deposed to by the witnesses, could not be reconciled with such an hypothesis.

I here append some extracts from the summing up of the learned judge, in order to show the manner in which the medical facts were laid before the jury.

"On the part of the prosecution it is alleged, that this young man, having formed an illicit connection with the unfortunate girl Jael Denny, and she being with child by him, had formed the determination of making away with her in such manner that it should be suspected and believed that she died by her own hand, and that he might escape with impunity. They allege, that in the execution of that wicked design, he formed an engagement with her at half-past six, on the evening of the 12th of October, and that having met her, he perfected that design. On the other side it is stated that she committed suicide; at all events, if murdered, that there is not sufficient evidence to bring home that act to the prisoner at the bar. Now, you are first to consider whether Jael Denny was murdered; and, secondly, if she was, whether the evidence satisfactorily proves to you that Thomas Drory was the murderer. You are not to act on mere suspicion or probability. You must be morally convinced of his guilt before you find a verdict of guilty. There was no human eye saw the offence committed. It is what is called a case of circumstantial evidence; but if there is that evidence before you which is altogether inconsistent with the innocence of the prisoner, then you are to find him guilty just the same as if he was seen by a person to do it. You must consider morally as well as legally the evidence as to the probability of her having committed suicide, and on that point there was this circumstance to be attended to; that the deceased, being near her confinement, had provided for the midwife;—her mother was to nurse her; and you are to say whether under these circumstances she was likely to commit this act. And you will not only take into consideration moral but medical circumstances, assisted as you have been by gentlemen eminent in science; and here one thing was most important; that the end of the rope was loose between the fingers of the left hand; and it must be observed that when these great crimes are contemplated it often happens, by the dispensation of Divine Providence, they are committed in such a manner that they lead to their own detection. The father said when the body

was found, there was blood about a foot from the side of the face, and how far that was to be explained on the theory of the deceased committing suicide, it is for you to say.

"On the morning of the 13th of October, when the mother said to the prisoner, 'You appointed to meet her at half-past six,' he did not deny it. It was for the jury to consider this. It was one of the most important points in this case; for if they thought there was that appointment, certainly the case assumed a different aspect. Spots of blood were discovered on the trowsers, and if they were spots of blood, it was very strange, that he should account for them by saying they were made by the stuff used for the calves. His Lordship then read through the evidence for the defence, and said he came at last to two witnesses, who, if they could believe them, would add materially to the supposition that deceased had committed suicide. Though their story was very extraordinary, if the wife had confirmed and corroborated the husband, it might have shaken their belief. They had heard what the wife said of it; the husband pledged his oath to the wife being present, but when they came to detail separately the conversation, respecting the intention of the deceased to destroy herself, it turned out to be totally different."

The jury returned a verdict of *guilty*.

Before his execution, the prisoner made a confession of his guilt, in which he stated that he met the deceased by appointment at the stile, about half-past six o'clock on Saturday evening; but in the meantime he had gone to a cellar in his father's house and taken from it part of a rope which was lying there. On meeting her for the second time, he said that he and deceased talked and walked about, after which, at her suggestion, they sat down on a bank. She had come to urge him to marry her. He passed the rope gently round her neck as they were sitting, and had got the end of it into a loop before she perceived it. She jumped up at once and put up her hands to save her throat, (at which time the marks of injury found upon them must have been inflicted,) but he pulled hard, and she fell without a struggle. He then left her lying in the field, and went to Brentwood.

Remarks.—In the above history of this remarkable case, I have not attempted to give a full report of the evidence of the witnesses, but rather an abstract of the principal facts proved at the trial, in order to throw light upon the medical questions on which the guilt or innocence of the prisoner depended. The medical evidence, however, on both sides, is given in full; and I am enabled, by the courtesy of the two medical gentlemen who appeared for the defence, to publish, in this paper, an account of the two cases which had led them to the belief that *this*

defence. While the two witnesses for the prosecution maintained that the act was *not* one of *suicide*, but of homicide; those for the defence went no farther than to assert,—the one that he thought there was “*a doubt*,” the other, that he should have “considerable difficulty in forming an opinion whether it was an act of suicide or homicide.” Thus, then, the witnesses for the defence could not say that in their judgment this was certainly an act of suicide. These opinions could hardly be said to be conflicting, because the witnesses for the defence merely declined giving a decided opinion one way or the other. Doubts and difficulties exist in nearly all cases in which medical opinions are required; they can only be resolved and removed by a most minute inquiry into facts; and, if witnesses have not had an opportunity of making such an inquiry, it is not to be expected that they can speak with decision upon an abstruse subject like that involved in this investigation. The cases which led these gentlemen to entertain doubts in reference to the act whereby Jael Denny lost her life, will, I think, be found to be easily explicable upon a comparison of the circumstances. As it will be seen hereafter, there was nothing in these cases incompatible with suicide; but there was everything incompatible with suicide in the case of Jael Denny, even supposing that no one had been charged with the murder. This, as it appears to me, is the true way in which to consider the question; for it is obvious that a medical inference from a series of appearances found on a dead body, ought not to depend upon, or to be modified by the fact of a person being charged with murder. An accidental circumstance of this kind may properly give rise to very great caution in drawing an inference; but it should not be allowed to have any other influence on a medical opinion. It is unnecessary to recapitulate the evidence given at the trial; but it may be proper to state in this place the series of facts and inferences which led me to give a very decided opinion, that this could not have been an act of suicide.

1. The deceased was *right-handed*; and on the hypothesis of suicide, she must have made the tension with her *left* arm and hand. From the position of the loop or noose, any traction to the right would not have tightened but have loosened the cord.

2. That, supposing her to have exerted such a traction at all she must have been in the erect or sitting posture. The force used, indicated by the great local violence to the neck, could not have been exerted by a person attempting to tighten a cord by drawing it to the left while in a recumbent posture, whether prone or supine.

This hypothesis would, besides, leave wholly unexplained the flattening of the nose, (obviously from direct pressure, not from

a fall,) and the fact that deceased had bled in two places, one spot being a foot from the other.

3. That the cord must have been pulled with excessive violence in a horizontal direction by one end only, as the mark was circular round the neck.

The other end of the cord formed a noose or loop, and was tightly fixed at the back of the neck. Thus, then, all the force of traction must have been exerted to the left, in which direction the right hand of a right-handed person could not act horizontally, so as to produce the amount of violence found on the soft parts of the neck.

4. That the fact of there being three coils and a half of rope round the neck, formed an obstacle to the tightening of the cord by pulling one end to the left so as to imbed the two inner coils in the skin, and to leave the outer or third coil loose.

On the supposition that the deceased produced the constriction by her own act, it follows that the three coils must have been round the neck at one time, and the two inner coils sufficiently loose to allow of respiration before traction was commenced.

5. The double indentation found on the trachea, could not have been produced by the two inner coils (on the supposition of suicide), except by the great tightening of the outer coil.

6. As insensibility and loss of power must have immediately followed the complete compression and obliteration of the trachea by the two inner coils, the outer coil ought not to have been found loose or unconnected with the object by which the force of constriction had been produced.

To suppose that the deceased could have produced the intense constriction by the first coil, and have afterwards retained sufficient power to pass a second coil from right to left around her neck, indenting the skin, and flattening the trachea as much by the second as by the first coil, involves, in my judgment, a physiological impossibility. There was, therefore, on the suicidal hypothesis, no explanation to resort to,—but that all the three coils had been placed at once round the neck loosely,—that one end only of the cord had then been so pulled to the left as to produce the great amount of violence found, and to tighten equally the two inner coils; while the outer coil and extremity of the cord, by which this immense force must have been applied to the two inner coils, was found lying loosely, without any attachment either to the hand of the deceased or to any other fixed point.

7. To have indented the neck, compressed and bruised the trachea in two distinct places; to have caused effusion of blood to the amount of a cupful from mouth, nose, and ears,—this effusion being found in two distinct places a foot distant from

tent of previous study and reflection. Hence, one who has only a half knowledge of the facts, or leaves some which are important, although minute, out of his consideration, cannot be in a position to form an opinion entirely free from doubt. In no other way can I explain the fact that a good anatomist and physiologist suggested to me a few days before the trial, that this act might have been one of suicide. This opinion was formed upon a comparison of some few reported cases, in which the circumstances, when strictly analysed, were so dissimilar as to justify no analogical application, except in the bare fact that death was caused by strangulation. It was undisputed that strangulation might be an act of suicide, but it was positively denied that, in this case, the act was suicidal.

The cases upon which Mr Thorpe and Mr Pollock, the witnesses for the defence, felt themselves justified in stating that the strangulation of Jael Denny might have been suicidal, are curious and instructive. These gentlemen have kindly favoured me with the following particulars of their cases, which I give in their own words.

Mr THORPE'S Case.—"C. H—, æt. 58, left his home at 7 A.M., for the purpose, as he stated, of going to London to present a bill. On his return home he effected self-destruction in the following manner; he passed a noose of cord over his head, and then inserted a stick, about fourteen inches long, between the cord and his neck. Having done so, he, with the assistance of the stick, twisted the end sufficiently tight so as to cause, I should say, almost immediate suffocation (strangulation?) Still it appeared there was time for him to insert the lower end of the stick in the inner side of the waistcoat, and the upper end was accurately adapted to the internal jugular vein and carotid artery. The date of this suicide was the 11th of February." (1851.)

In this case, it will be seen, the deceased committed suicide by employing a circular ligature and tightening it with a stick, on the principle of the tourniquet. The cord was tightened by the twisting of the stick sufficiently to obstruct respiration, and the stick was retained in the inside of the waistcoat in such a position as to prevent it from slipping or loosening the cord. Here, then, was a fixed point found for the constriction, and sufficient to explain it. There was nothing in the position of the stick or the cord incompatible with the fact of the man having perpetrated the act himself. There was no more violence to the neck than the stick and cord could have produced by the deceased's own act. There was no effusion of blood from nose, mouth, and ears. The man was not found with his dress torn, his nose flattened, and other marks of recent violence on his

person. Had these appearances existed on the body, and had the stick been found loose or lying at a distance, with no fixed point from which the constriction could have originated, it is probable that the opinion of suicide would not have been entertained in this instance, or at any rate considerable doubt would have arisen whether it might not have been an act of homicide. As it is, the case merely proves that a person may strangle himself by the aid of a noose and a stick ; and there are several such instances on record. In no one point, except in the cause of death being the same, does it bear any analogy to the case of Jael Denny.

Mr POLLOCK's Case.—"Pizzalla, an Italian, about fifty years of age, employed as a porter, was found dead in the forenoon of the 3d January 1851, in an attic of the house of his employer. He had been missing from his employment thirty hours. When found he was lying on his back, rather inclining to the left side, with a piece of ordinary sash-line coiled four times around his neck, two of the coils so tight and imbedded therein that there was some difficulty in undoing it. The right hand held one end of the line, and the left hand the other, with a turn of the line around each to hold it the more securely. The right arm was extended, the left flexed. I made a *post-mortem* examination of the body on the fourth day after it was found. Externally, the face was swollen and purple, the vessels of the conjunctivæ were congested, the tongue protruded towards the left side, bloody froth issued from the mouth, and the lower jaw was slightly twisted towards the left side. The skin of the neck was abraded in a nearly continuous line around it, about five-eighths of an inch in width, and presenting the appearance of being produced by two coils of the line. There was considerable ecchymosis above and below the line of abrasion. Each hand retained the impression of the line being coiled around it. Internally, the vessels of the brain and its membranes were greatly congested.

"The evidence before the coroner left no doubt of this having been a suicidal act."

This case proves that a person may strangle himself and that he may accomplish strangulation by pulling the two ends of a cord coiled several times round the neck ; and that some degree of local violence to the neck may thus be produced by the ligature used. These are points which could not reasonably be disputed, even supposing that no such case as that of Pizzalla had occurred. In what way, however, would the facts of this case justify a medical opinion that the deceased, Jael Denny, had destroyed herself? She was on her face, with her nose flattened and other marks of violence on her person. The cord was not held in or wound round each hand, and there was no mark or

nothing medically or morally in favour of homicide. As to the third, it may be stated there is nothing to prevent a person tying two knots in a handkerchief, provided the ligature be not drawn so tightly round the lower part of the neck as to compress and obliterate the trachea. No ecchymosed mark had been produced on the neck ; a fact which shows that the constriction could not have been at any time very great, and the part of the neck, included in the ligature, is not stated. As the ligature was secured by knots, it did not require tension by the hands. There was no violence on the body, and there was nothing in the case incompatible with the act of suicide. Except that the deceased had died from strangulation, there was no analogy whatever in the circumstances to the case of Denny. This is substantially stated at p. 717 of the work from which the learned counsel quoted.

In the second case, reported in the *Annales d'Hygiène*, 1829, vol. ii., p. 440, the deceased, a female, was found lying in bed on her face, and one of her garters passed twice round the middle portion of her neck, and secured by two knots in front (*i. e.*, one knot tied strongly upon the other). There was only a slight or superficial mark produced by the ligature, rendered apparent by a few ecchymosed spots. This was perceptible in the fore part, but scarcely visible behind. The face was swollen and livid, and the tongue strongly clenched between the jaws ; a sanguineous mucus escaped from the mouth and nostrils. There was no disorder of the dress, bed, or furniture, and no mark of violence on the person. On dissection, there was no subcutaneous effusion, and no violence to the muscles between the skin and the larynx. The larynx itself, and the upper rings of the trachea presented no marks of injury. The examiners came to the conclusion that this was an act of suicide.

It is obvious, from the description, that the seat of pressure here was high up, and embraced the lower part of the larynx. The constriction was not sufficient to produce a deep mark, and those who inspected the body expressly state, that, although the two knots were well secured, the first ligature itself had only been drawn with moderate force (*mediocrement serré*) round the neck, so that, in their judgment, it had caused death by interfering with the cerebral circulation only, producing apoplexy, and not by suddenly interrupting respiration and producing asphyxia. Under the circumstances in which the body was found, considering the seat of pressure, and the small amount of pressure as determined by the dissection, it was quite possible for the deceased to have tied two or even three knots in the garter. The circumstances in Denny's case were so entirely different, as

compressed that the tube is flattened, may take place in a few minutes. Insensibility, loss of consciousness and power to make any muscular exertion, cease almost immediately; hence, it was strongly alleged in this case, that after one constriction of the neck had been made in the degree proved by the amount of violence, a second constriction could not be made by the individual; he would lose the power and consciousness necessary for such a muscular exertion. Both the witnesses for the prosecution and defence concurred in the view, that insensibility would be an immediate result of a coil of rope being tightly drawn around the windpipe; and all agreed that when the pressure was at the upper part of the throat above the windpipe, insensibility is not so soon produced as it is when the pressure is at the lower part. In fact, it is generally admitted by physiologists, that the throat can bear greater or longer continued compression at one part, without producing unconsciousness, than at another. Above the larynx, the mechanical disposition of the muscles and of the parts about the fauces prevents that direct stoppage of the act of respiration, which would immediately follow when the same degree of constricting force was applied to the trachea.

On this subject, the experiments of Fleischmann made upon himself are of great interest. It was to them I referred in my evidence. This gentleman found that when he placed a cord round the neck, in the space just below the chin, it might be strongly drawn, either laterally or behind, without the act of respiration becoming perceptibly disturbed, and a person may, under these circumstances, inspire and expire for a long time. This is what might have been anticipated, since a ligature so placed nowhere compresses the air-passages. But under this constriction, the face becomes red, and the eyes congested and protruding; the head feels hot, there is a sense of weight, followed by vertigo, and there is suddenly a hissing noise in the ears. This last is a symptom of impending danger; and unless the experiment be discontinued at this time, the result may be fatal. These effects show an interruption to the return of blood from the head.

Similar effects are produced when the cord is applied over the larynx. In this case the symptoms come on with greater rapidity, and there is from the first some difficulty in breathing. The previous experiments could be carried on for two minutes; but in this case, scarcely thirty seconds had elapsed before the hissing noise in the ears came on, and with this an indescribable sensation in the brain. The variation in the seat of pressure, explains the difference in the latter case; there would be a greater mechanical interference with the act of respiration, and a greater obstruction to the return of blood from the head.

than the natural hue. The pulse is rapidly enfeebled, and is sooner extinct,—first in the extremities, and afterwards in parts nearer the centre of circulation.” (On Asphyxia, p. 70.)

These facts regarding the effects of even a moderate compression of the air-passages in rendering a person rapidly insensible and powerless serve to explain how it happens that, in numerous instances of suicide by hanging or strangulation, the bodies are found kneeling, lying, or otherwise supported in a position in which the suicides might be supposed fully capable of moving, and thus of saving themselves. But insensibility is most insidiously and most rapidly induced, and a partial circulation of imperfectly arterialised blood suffices to prevent not only muscular exertion, but even the consciousness of the necessity of making it. Dr Jaquemain examined a case of suicidal strangulation in which the deceased had passed a cord round his neck, and secured the other end to a nail in the wall a little above his bed.* When the body was found, it was partly inclined against the wall in a kneeling attitude. The two feet rested on the bed. The constriction in this instance was on the neck generally, and it was only partial, as the weight of the body was, in a great degree, supported. In a case which occurred to M. Esquirol, a female lunatic passed a cord round her neck, fastening the other end to a short post on an embankment. She had then allowed her body to slide downwards. Although seen to commit the act, and assistance was instantly rendered, she was found to be quite dead by the time the attendants reached the spot. Instances of this kind might be multiplied, but these will suffice to show that insensibility and powerlessness to move or alter a position, are very rapid effects of a general compression of the neck when respiration is even only partially impeded.

In hanging by the whole weight of the body, the line of constriction is generally above the larynx, owing to the slipping of the cord; hence the pressure on the air-passages is not so complete as in strangulation by close constriction of the trachea. Nevertheless an individual is speedily rendered insensible and powerless even under these circumstances, doubtless from interruption of the return of blood from the head. I am indebted to Mr B. W. Richardson, of Mortlake, for the following singular case, in which the individual committed suicide by a mixed condition of hanging and strangulation.

“On the 2d of May, 1851, between 7 and 8 o'clock a.m., Mr Beresford was summoned to the village of Tibbesthorpe, near Leicester, to see a farm-servant who had hanged himself. On arrival he learnt that the occurrence had taken place in a barn, and found the man supported in the sitting posture, and not

* 407 Ann. d'Hygiène, 1831, i. 202.

examination was most unfortunately not made. I should just add that Mr Beresford and another gentleman who was in attendance, Mr M'Keller, are of opinion that there was a ruptured cranial vessel, produced, perhaps, by the fall on the ground during the extreme degree of congestion that must have been produced by the suspension.

"The man was 22 years of age, and of short stature."

The fact, that insensibility and loss of power immediately take place from a complete compression of the trachea, and consequent stoppage of respiration and cerebral circulation has been long known from the murderous acts of the Thugs in India, as well as from some recent crimes of a similar description perpetrated in the streets of London for the purposes of robbery. The constriction of the trachea serves the purpose of the assassin and robber, by not merely rendering the person attacked immediately insensible, but by putting it out of his power either to make any cry whatever for assistance or to offer any resistance. It is the most secret, silent, and rapid means of murder available to the assassin; and hence its recent employment for robbery calls for the severest punishment which the law can inflict. Insensibility requires only a few seconds for its production; and death is the inevitable result, if the constriction be continued in full force for one or two minutes.*

As a curious instance of the ignorance which prevails on this subject among persons who, by education, are supposed to know better, and who profess to instruct the public on questions appertaining to physiology, I subjoin the following letter, which was forwarded to and published in one of the daily papers after the trial and conviction of Drory, and before his confession of the crime, which the writer evidently wished the public to believe might have been an act of suicide.

"Sir,—I think it somewhat doubtful that immediate insensibility is invariably the result of close compression of the trachea. I was once called to a case where a man was found strangled in a bush in the following way:—He was riding home drunk on the top of a cartload of potatoes; he fell asleep; the horse walked on, approached nearer and nearer to a ditch, and at last one wheel slipped in; the man fell over, and his neck caught in the angle between two branches, and then the uppermost branch was pressed closely to his neck by the wheel falling upon it and holding it down. In this position it was evident that the man had endeavoured to liberate himself by taking his knife out of his pocket and

* In capital punishment as carried out by the law of Spain (the *garrote*), we have an instance of the rapidity of death by strangulation, when there is a close constriction of the trachea. A collar is placed round the neck, which is capable of being tightened by a screw from behind. In the late execution of General Lopez by the *garrote*, at Havannah, his head is stated to have dropped forward, and he was lifeless simultaneously with the first turn of the screw.

trying with it to cut away the branch. In that he had failed; but the taking the knife out of the pocket, the opening it, and the three or four cuts made with it must have occupied some time. The man's arm was stiffened in the position in which it must have been when trying to cut the branch. There was no mistake as to the amount of pressure to which the neck was subjected; for the dead body could not be released without the assistance of an axe, and the branch, so thick as to require an axe to divide it, was held down by the whole weight of the cart—I am, Sir, your most obedient servant.

“*Alford, Lincolnshire, March 11.*”

“R. U. WEST, Surgeon.”

I pass over the absurdity of appealing on such a question as this to the daily press. The letter itself contains unanswerable evidence of the great difficulty which the writer has experienced in drawing inferences from facts, or even in so recording facts as that just inferences may be drawn from them by others. In the case of Jael Denny there was immediate compression of the trachea in two distinct spots, so complete, as to bruise the tube, and, from the appearances, to obliterate the canal temporarily. In this instance of accidental strangulation, the neck, we are informed, was subjected to pressure, by being caught in an angle between two branches of a tree, and pressed down by another branch from above. The angular space into which the neck fell could not, from this description, have produced immediately if at all, that complete obliteration of the trachea which would be certainly effected by a tightly drawn cord; at any rate, there is nothing to show that such compression would be so instantaneous, as it must have been, by the application of a cord, in Denny's case. The man might, therefore, have breathed slightly for some time, and thus have been able to employ muscular exertions to release himself: the very fact of these exertions having been made, proves that some degree of circulation and respiration must have been going on, and the supposed analogy, therefore, of this case to that of Denny entirely fails. The writer of the letter must either believe that a person on whose trachea a close double ligature has been forcibly applied, might afterwards occupy “some time” in making cuts at the branch of a tree, or his case has no bearing whatever on that of Denny. The application of a ligature to the neck of a dog or cat, so as to include the trachea completely, would have shown him the irrelevancy of the case which he adduces, and the error of the inference to which it would lead. So far as the details of his case are given, it does not appear that it was the trachea, but the neck generally, that was subjected to close compression.

In an experiment performed since this trial, a dog was strangled, not by an amount of pressure “on the neck,” but by a ligature directly applied to the lower part of the neck, includ-

ing the trachea. The animal was instantaneously rendered insensible, unconscious, and incapable of any muscular exertion. The rapid circulation of the unarterialised blood fully accounts for this result.

The evidence in the case of Jacl Denny clearly proved that the prisoner had had sufficient time to commit this murder by strangulation. An hour and a-half had elapsed from the time he left his house until he was seen by the witnesses at Brentwood. The distance was about two miles, and might have been walked in half an hour. He set out with the basket of eggs, with the professed intention of going thither; he had, therefore, a full hour at his disposal, but such an act of murder would require only two or three minutes for its complete perpetration; hence, if the time accounted for had not been more than three-quarters of an hour, and the distance from Brentwood even greater, all the medical bearings of the case would have been fully satisfied. There is no death so rapid as that which takes place from strangulation, when it is accomplished by such means as those proved to have been employed in the case of the deceased.

5. Evidence affecting the accused from stains on his clothes.—

When the prisoner was arrested, some dark stains were observed on the front of a pair of corduroy breeches which he wore, and which he admitted he had worn on the evening of the day that he went to Brentwood. The evidence of the superintendent of police was to the following effect:—

“The blood appeared quite fresh, and the patches were moist or damp. I pointed them out to the prisoner, and asked how they came there. He said it was not blood, but some stuff he had had to give the calves, and if I went with him to his house, he would show me the pot where it was kept. He took me to an outhouse, and, pointing to a pot, said, ‘that is the pot where the stuff is.’ On taking it down from the shelf, the mouth of the pot was open, and covered with cobwebs. There was a wooden spoon in it, the handle of which did not reach to the edge of the pot, but was quite inside. On showing the prisoner the cobwebs on the mouth of the pot, he said he had not used any of the stuff for a long time.”

The stains in front of the small clothes were not so well marked in colour or appearance as some small spots at the back part, in the bend of the ham; these had evidently escaped notice. The stains in front were of a red-brown colour; they had evidently been wiped, so that the blood had been removed from the prominences of the cords, and carried into the deep grooves of the stuff. It was in this situation, and by the aid of a powerful magnifier, that small solid coagula were seen, having the colour

microscope, blood-discs or corpuscles were seen presenting all the usual characters of mammalian blood. Dr Rees examined a portion separately, and came to the same conclusion as myself.

Human and Animal Blood.—As it was considered necessary to determine whether the blood-stains on the corduroy were caused by the blood of an animal, or by that of a human being, another investigation was undertaken, in order, if possible, to form an opinion on this point. Chemical tests utterly fail to establish any distinction; and after many trials of Barruel's process of acting on the blood by sulphuric acid, I can come to no other conclusion than that it furnishes no criterion whatever, and that it would be dangerous to rely upon it in any case. Barruel's process is based on the hypothesis that the blood of every animal contains a peculiar volatile principle intimately combined with it, but capable of being set free by the addition of concentrated sulphuric acid, and then possessing the odour of the perspiration or sweat of the animal from which the blood has been taken. We are told that, *cæteris paribus*, the odour thus evolved, is much more marked in the male than in the female animal, and that in man the colour of the hair appears to exert some influence on the strength of the odour! To produce the result, one measure of blood is mixed with one and a half measure of concentrated sulphuric acid; the mixture is well stirred with a glass rod, and the odour is perceived during the act of stirring. It will be noticed, that the employment of this process presupposes that we have fresh and liquid blood in some measureable quantity, with which we may operate. It is quite inapplicable to blood that has been kept for some time, or to small and scarcely perceptible stains on articles of clothing. Dr Carl Schmidt, of Dorpat, who has lately examined this process, states that, under favourable circumstances, the blood of the goat and the cat may be recognised by the evolution of the peculiar and well-marked odour of these animals; but that in all other cases it gives doubtful, and therefore untrustworthy results. I lately had an opportunity of examining, by this process, the fresh blood of the dog, taken from the vena cava very soon after death. The result was, that it presented no particular odour which could be referred specially to the dog.

Microscopical differences.—An attempt has been made to draw a distinction between animal and human blood, from the different size of the corpuscles. The blood of birds, fish, and reptiles may be distinguished by the shape of the corpuscle; in these three classes of animals, when the corpuscle is in its natural condition, the form of the disc is oval or elliptical; in the human

being and in mammalia generally (excepting the camel tribe) it is round. These blood-stains were clearly not caused by the blood of birds or fish, as they presented the well-marked rounded form of the mammalian corpuscle. It was suggested in the defence, that the stains on the clothes might have been produced by the effusion of blood while the prisoner was engaged in killing chickens; but microscopic observation showed that the blood was of the mammalian variety, and therefore this suggestion would not explain the facts.

It is well known, from the researches of microscopists, that the blood of one mammal differs from that of another, in the fact that the corpuscles are larger in some and smaller in others. This, however, is only an average difference; *i. e.*, in the same blood, corpuscles of very different sizes are constantly found; hence, in making use of this criterion, it would be necessary to rely upon the size of the majority of the corpuscles seen in a given area, and under the same power of the microscope. It is a curious fact that the size of these corpuscles in the blood, bears no relation to the size of the animal. Thus, in the horse, ox, ass, cat, mouse, pig, and bat, they are, on the average, of nearly the same size; the difference is so slight as to be practically inappreciable. In these animals they are smaller than in man, and several of the mammalia. The corpuscles in man, the dog, and rabbit, are of nearly the same size. In the blood of the goat they are much smaller than in other mammalia. The size of the corpuscle bears no proportion to the age of the animal; thus in the blood of the human foetus they are to be found as large as in that of the adult.

The measured diameter of the corpuscles, according to Gulliver, is, in the elephant, 1-2745th of an inch; in the dog 1-3540th; in the hare 1-3607; in the rhinoceros 1-3765th; in the mouse 1-3814th; in the ass 1-4000th; in the pig 1-4230th; in the ox 1-4267th; in the cat 1-4400th; in the horse 1-4600th; in the sheep 1-5300th; in the goat 1-6366th; in the human blood, on the same authority, the diameter of the corpuscle varies from 1-2000th to 1-4000th of an inch; and the average size in both sexes is 1-3200th of an inch.

In microscopical observations, under a power of from 216 to 340 diameters, made on the blood of a dog recently killed, I found that the corpuscles had the usual round form of the human corpuscles. The greater number of these examined in the serum of the blood gave a measurement of 1-4000th of an inch in diameter. A few were larger, but many were smaller; some had a diameter of the 1-6000th of an inch, while the smallest perceptible was the 1-8000th of an inch in diameter. For this experiment the blood had been collected ten minutes after death,

evidently soaked through the stuff, but the spot was much smaller than on the outside. In most of the stains at the back and front the appearance was confined to the outside only; hence it was clear that the clothes must have been stained from the application of blood on the outside.

Living or Dead Blood.—Had the blood which had caused these stains been effused from a dead or living body? From the fact of distinct clots or *coagula* of blood being present in some of the stains, and having a glistening and bright appearance, at the same time adhering firmly to the fibre of the stuff which was stiffened by imbibition of the serous or more fluid part of the blood for some space around, it was inferred that the blood which had caused these stains must have come from the body of a living person or animal, or of one but recently dead, and in whose body coagulation had not taken place. In drawing this inference it is necessary to bear in mind that the already coagulated blood of a dead body may dry in a clot on clothing on which it is accidentally spilled or thrown, and give a slight stain to the parts around. In this instance, however, an examination by the microscope rendered it highly probable that the blood had fallen on the stuff in a liquid form, and had there coagulated. Each pointed fibre of cotton was seen to be invested with a separate coagulum, of a rounded or pyramidal shape, closely adhering to and stiffening the fibre. It is well known that pointed substances presenting a large surface greatly accelerate the coagulation of recently effused blood from the living body. The nature of the clothing was therefore well adapted to produce this rapid coagulation, and to fix the blood on the fibres immediately after its effusion. The answer returned to this inquiry was, that the blood had been effused from a living person or animal, or from one but recently dead.

How long had the Blood-stains been on the Clothing?—By sprinkling a portion of freshly-drawn human blood, as well as that of a rabbit recently killed, on an unstained part of the prisoner's clothing, it was found that after the elapse of twelve hours the blood had produced a dingy red-brown stain in each place, resembling the suspected stains on the clothing, and not to be distinguished by physical appearance or microscopical observation from them or from each other. After the lapse of twelve hours they underwent no further change in colour or appearance, when observed at intervals during many weeks and for several months.

The prisoner was arrested by the superintendent of police about twelve hours after the period at which the deceased had died. The dark-coloured stains in front of the corduroys were observed by him to be then moist or damp. There was reason to believe,

a series of experiments by sprinkling an unstained portion of the corduroys with solutions of the various red colouring matters known to chemists. Professor Morton, of the Veterinary College, informed me that the only red colouring matters used as astringents by farriers of the old school of veterinary practice, were Armenian Bole and Dragon's Blood. The former is an insoluble mineral compound, owing its colour chiefly to peroxide of iron; and the absence of this was clearly proved by the chemical examination of the stains. Dragon's blood is a resin insoluble in water, but soluble in spirit; it is used as a styptic. In addition to these two substances, red Sander's wood, kino, and catechu, are also employed.

I am not aware that the changes produced in red colouring matters, by a common article of dress dyed with oxide of iron, has ever before been a subject of remark. I therefore subjoin the results obtained from various experiments:—1. Brazil wood. The stains produced by a decoction of this wood presented when dry a dark puce-brown colour. The stain was of a uniform tint throughout; it presented no appearance resembling a clot or coagulum of blood. Examined by the microscope, the colour was wholly different from that of blood, and could not be mistaken for it. The colour of the stain as well as of the decoction was rendered crimson by weak ammonia; the colour was altered but not removed by diluted muriatic acid. 2. Logwood. The stains were of a dark slate colour; the colour of the decoction was altered by acids, and rendered crimson by weak ammonia. Under the microscope there was no appearance which could be mistaken for the colour of blood. 3. Red Sander's wood. Stain of a dark red colour; altered by acids, and rendered crimson by ammonia; no stiffening of the fibre; no appearance of coagulum; not soluble in water; no change when touched with a persalt of iron. 4. Madder. This produced a dark brown stain on the stuff. Decoction and stain turned crimson by diluted ammonia, and changed to a bright rose-red by a solution of weak muriatic acid. The decoction was darkened by a solution of persulphate of iron. 5. Alkanet. This substance was scarcely so dissolved by water as to give to the liquid a red colour, even when boiled for an hour. It produced a dark brownish stain, bearing no resemblance to blood; there was no stiffening of the fibre, and no coagulum. Ammonia gave a greenish, yellow colour. Muriatic acid destroyed the colour. Persulphate of iron gave an olive-green tint. 6. Archil. The stains produced were of a dark purple-red colour, locking up particles of gritty matter. No stiffening; no appearance of coagulum; colour changed to blue by ammonia, and to bright red by acids. 7. Cochineal. A strong decoction of this beautiful red dye produced a stain almost black.

and in that of M. Bricheteau, at the Hospital Necker, eighteen patients, of whom five were labouring under very acute rheumatism, eleven under acute rheumatism, and two under moderately acute rheumatism. This division rests upon the degree of intensity of the fever and of the pains, and that of the general distribution of the disease. The results obtained may be distinguished into primary and consecutive.

The primary results are the following. Complete tranquillity immediately after each application, varying in duration from one hour to six or eight hours, according to the acuteness of the disease, and more especially according to the acuteness of the febrile condition. It is requisite, says M. Aran, to have witnessed the change induced by these applications, in order to comprehend the amount of their importance. Patients fixed in a state of immobility by their pains are enabled immediately to execute movements and turn themselves in bed. Under their influence, sleep, which was previously disturbed and gone, returns, and a considerable number of patients sleep immediately after the applications. Further, at the same time that the pain is diminished and disappears, the swelling, the redness, the intra-articular effusion disappear also the ensuing day; in other instances, nevertheless, only a diminution in these morbid phenomena takes place. M. Aran thinks also that he has observed that the articulations, which have been detumefied by means of local applications, have little tendency to be again attacked by the disease. *Lastly*, the febrile movement generally declines, at least from a general condition, or from an inflammatory complication.

The definitive results are the following. Medium recovery; for attacks of very acute articular rheumatism on the tenth day of treatment, or on the eighteenth day of the disease; for attacks of acute rheumatism on the seventh day of treatment, or on the fifteenth day of the disease; for attacks of moderate acuteness on the sixth day of treatment, or on the sixteenth day of the disease.

Among eighteen cases of rheumatism, ten were complicated at the instant of the commencement of the treatment, with endocarditis, old or recent, namely, the five cases of very acute rheumatism, and five cases of acute rheumatism. Of these ten patients, five presented complications towards the chest; among the five patients, two had already an organic affection of the heart, and four had one or several attacks of rheumatism.

The alterations observed were, in two instances, a twofold pleuritic effusion; in one case, a pleuro-pneumonia, and in one case, a double pleuritic effusion with pericarditis. All these five

cases got well under the employment of blood letting and the application of blisters.

From these facts, M. Aran deduces this conclusion, that, in the instances in which the febrile movement is very acute, there is ground to employ, along with the local applications, general blood-letting. In this manner he treated, with one or two blood-lettings in the course of twenty-four hours, and with anæsthetic applications upon the joints, five patients, one of whom was labouring under very acute rheumatism, and four under acute rheumatism, producing a complete cure in nine days. M. Aran, in short, allows that this mode of treatment by local application, does not exclude other approved therapeutic methods.

At a subsequent meeting, on the 23d December, M. Aran communicated to the Academy, upon local anæsthetic treatment, a note containing several important facts.

M. Aran refers to the experiments of MM. Serres, Flourens, and Longet, in proof of the proposition, that anæsthetic agents exert upon the nervous chords a local influence in which they extinguish sensibility. But this was scarcely requisite; for it is well known, for a very long time, at least since the days of Robert Whytt and Francis Home, that anæsthetics do act locally, whether upon the nerves or not. He refers also to experiments performed by Dr Simpson and other physicians, both in England and France, in proof of the inference that by means of local anæsthetic applications, muscular rheumatic pains and neuralgic pains are alleviated. The success obtained encouraged M. Aran to continue his researches, and he has obtained results, which, though not complete, may probably serve as a foundation to new applications and further inquiries.

It has been seen that M. Aran did not use, and does not recommend the use of chloroform. He found, in short, that there was a peculiar difficulty in making use of this agent for local anæsthetic application. Chloroform produces a sensation of scorching and intolerable burning so severe, that patients obtain relief by very acute sufferings, and sometimes even after sustaining a burn in the first or in the second degree. Anæsthetic properties in different degrees are found to reside in a very great number of hydrocarbonated and chloro-hydrocarbonated substances. After having tried these different substances, his attention was attracted by a particular product discovered by M. Regnault, and to which this learned teacher has given the name of *chlorated* chlorhydric ether. On this subject M. Aran received from M. Mialhe a note, of which the following is the most important portion.

rate in the nerve, sensibility from the motific power; and the other separate in the muscle, the force which causes rigidity, which causes tension, from the force which induces relaxation.

The same experiments appear further to establish a line of separation between muscular action and nervous power or energy; for, on the one hand, tetanic rigidity appears at the very time when the motific power of the nerve is lost, and on the other hand, muscular relaxation is established at the time when the moving power of the nerve subsists. There is therefore a visible independence between the action of the nerve and the action of the muscle. These experiments M. Flourens, justly remarks in concluding, furnish a new means of physiological analysis, and probably that which is the most delicate and the most efficient that physiologists have yet been able to employ.

ART. VII.—*On Fatty Diseases of the Heart.* By RICHARD QUAIN, M.D., &c. (Medico-Chirurg. Trans., Vol. xxxiii. No. ix. p. 121.) London, 1850.

DR QUAIN arranges the facts which he has collected on this subject in the following order:—

- I. The varieties of Fatty Diseases of the Heart, and their respective characters.
- II. An account of preceding observations on these Diseases.
- III. A consideration of the circumstances under which they occur—their causes.
- IV. Their effects on the Structure and Functions of the Heart.
- V. Their Symptoms and Diagnosis.
- VI. The Indications for their Treatment.

Appended are found Tables containing the abridged histories of eighty-three cases, in which fatty disease of the heart existed. Of these cases twenty-five are derived from original sources,* and the others from different publications, with additions, in several, derived from inquiries made of the authors. Drawings illustrative of the morbid conditions described are also annexed.

I. THE VARIETIES OF FATTY DISEASES OF THE HEART, AND THEIR RESPECTIVE CHARACTERS.

There are two forms under which fat presents itself as a disease of the heart. In one of these forms the fat, composed of large

* A few of the cases thus obtained have appeared in the journals from time to time since they were placed in these tables. Mr Paget's and Dr Ormerod's valuable cases have been published so recently, and in so distinct a form, that Dr Quain thought it not necessary to introduce any of them here.

others. The fat may so completely envelope the heart, that none of the muscular tissue can be seen on its external surface. A mass of fat of this nature may of itself be sufficient to oppress and embarrass the heart's action; but fat rarely exists in such abundance on the surface of the organ without insinuating itself between and encroaching on the heart's fibres. In this way the muscular parietes of the organ become thinner and thinner, until the columnæ carneæ appear to arise from a mass of fat, as they are described to do by both Laennec and M. Bizot. This state constitutes what some writers have regarded as fatty degeneration, what M. Rokitsky has called "fatty metamorphosis," but which is, in reality, nothing more in many cases than an hypertrophy of fat. In hearts or parts of hearts less affected, that is where the fat is not very abundant, simple striæ of yellow tissue will be observed lying amongst the fibres. This appearance is common in the auricles.

When portions of the heart suffering from fatty growth in a high degree are examined with the microscope, it will be found, that where the growth is most advanced, that is always the external surface, few muscular fibres can be seen, and the wide intervals between them are occupied by fat cells. Proceeding inwards, we find the fibres become more evident, the fat cells fewer, and finally we have the fibres beneath the endocardium with a few cells lying here and there amongst them. It is deserving of attention, that the fibres, even though they are overwhelmed with fat, may still retain their organization; but in all cases, the course and direction of the fibres are more or less modified and distorted. Thus, then, there is of necessity neither a degeneration nor metamorphosis of the muscle, but a growth or hypertrophy of fat upon it, though in many cases some of the fibres will be found degenerated. The fibres still existing, though concealed, explain the persistence of the heart's action in those cases in which the muscular walls appear to a greater or less extent replaced by fat.

A curious appearance, sometimes presented by the fatty tissue forming little masses within the heart, should be mentioned here. These little masses appear beneath the endocardium, and are raised above the surface of the ventricle. They are in size from that of a pin-head to that of a pea, and they have been accurately described by Lancisi,* M. Bizot,† and Dr Peacock. I have seen these little fatty tumours in two cases. There was a considerable but not an excessive amount of fat on the surface of the heart in both these cases.

b. Fatty degeneration of the heart's texture.—The general characters of this morbid change will be more readily comprehended

* *De Motu Cordis Romæ*, 1728, p. 55.

† *Mémoires de la Société*, &c., p. 357.

after the alterations which the fibres undergo, as shown by the microscope, have been described. When a portion of the heart thus diseased is examined, the first thing which meets the eye is a want of the transverse striæ which mark the fibres of all the voluntary, and less distinctly those of the heart amongst the involuntary muscles. When a portion of the object in which the disease has but commenced is examined, it will be observed, that in addition to the faintness with which the cross markings are seen, a number of dark small dots stud the fibres in many points, evidently situated within the fibre. A few such dottings do not appear inconsistent with a tolerable healthy state of fibre, and they are frequently found in hearts which have well performed their functions. As the observer, however, approaches the seat of more decided disease, he finds that the cross markings have in many points disappeared, and that the black dots have increased in size and number; some of the dots are now found to be transparent in the centre, and to assume some order in their arrangement. Sometimes the centre of a fibre is occupied by a long row of opaque points, forming a continuous line in the course of the filament, sometimes two or three lines lie side by side, the dark lines being broken by transparent apertures or circles. Less frequently the lines lie transversely to the fibre. These appearances are well represented in fig. 3, Plate III.* Thus may be traced, as Mr Paget has described, the change from the particle of molecular fat, the black dot in the fibre to the large oil globule with its transparent centre. These granules and globules become more numerous, they finally occupy the whole fibre, and cause it to present the appearance well shown in fig. 2, Plate III., or in a still more marked manner by a higher magnifying power, and in a more advanced form of disease, in fig. 1, Plate IV. The fibres, then, as shown in fig. 2, Plate III., present much the appearance of, and cannot be compared to a more like object, than the tubules of the kidney, when the cells of that organ are the seat of fatty degeneration. This fat, be it remembered, is not in the ordinary fat-cells, such as are found on the surface of the heart or amongst the fibres. The fat globules placed within the fibre are much smaller, appear to have a mere albuminous envelope, and are extremely like the oil globules of milk. They often escape from the broken fibres, float as free oil globules, or lodge between the fibres, and give the appearance, as M. Rokitsky believed, of being partially placed external to them. Mr Paget and Dr Ormerod have fully confirmed the observation, that they are situated within the fibre itself. It will be further observed, that the diseased fibres are so friable, that they break up readily into small disunited fragments or short masses. In making a section, it is sometimes possible to obtain a small diseased point occupying

* These references apply to the Plates in the *Medico-Chirurgical Transactions*.

monia. This state more frequently occurs in hypertrophied hearts. These differences in the amount of change in the colour and consistence appear to be connected with the causes on which the diseased condition depends, when this can be referred to a local modification of nutrition—such as obstruction of the coronary arteries—a small portion only of the heart may be affected but in the highest degree. It is then that the fatty degeneration occurs to a limited extent, but in its most marked form. Such is the condition which appears to have been noticed by Laennec particularly. When, however, the origin of the disease can be referred to a more extended lesion of the nutritive function, a greater extent of the heart may become diseased, but in a less degree. In the one case we have complete and extreme fatty degeneration, in the other merely the pale soft flabby heart, which has been described as granular degeneration of the organ. Thus, then, we have two forms of degeneration of the muscular fibre fundamentally the same, and differing merely in extent and degree. Between these two forms I make no essential distinctions, save those of extent and degree. In addition to the changes of colour and consistence, we also may remark other appearances in connection with this morbid process. The fibrous character of the heart's structure even to the naked eye disappears, and in some cases the tissue resembles that of a fatty or boiled liver. In other cases, the cut or torn surface has a granular aspect not unlike that of the surface of the lung in an early state of grey hepatization. These different appearances may be in some measure due to the greater or less fluidity of the oily matter present, as well as to the extent and degree to which the disease has advanced. In one case which came under my notice during frosty weather, the torn surface of the fibres presented the granular glistening character of the fractured ends of a piece of steel. Varieties also in appearance are caused by the presence of a greater or less quantity of blood or its colouring matter in the heart's texture or in its cavities, by which the lining membrane may, in the latter case, be dyed of a deep purple colour. These lesser modifications do not, however, interfere with the leading characters of the disease, which are paleness, softness, peculiar mottling, and friability of the heart's texture, changes which, with the altered character of the fibres as shown by the microscope, will leave no doubt, when present, of the existence of this morbid change. These alterations must, however, in some cases be looked for with care. In those cases in which the disease is much advanced, the quantity of fat present is so considerable, that the greasy appearance is at once evident to the naked eye. The scalpel used in cutting the tissue is greased, and so is blotting paper when applied to the cut surface. In other cases, however, in which the disease is far more

was long recognised before the actual changes in the tissue were described by Mr Bowman,* so this fatty degeneration of the heart had been recognised by Laennec and the writers above named, before its true character was ascertained. Laennec, however, was wrong in saying that the disease proceeded from without inwards, and also that it was confined to the apex of the heart.

MM. Bouillaud,† Andral,‡ Grisolle,§ and other French writers seem to have adopted the views of Laennec, and to have compared this change, without inquiry, to the fatty degeneration of the voluntary muscles. The increasing accuracy required in pathological investigations, and the improved appliances for conducting them, soon rendered this ambiguous term of comparison

and more analogous to ligamentous tissue. I have examined this so-called fatty degeneration of voluntary muscles in four cases.

1st. The fatty muscle of over-fed prize cattle,—that which Vicq D'Azyr states that Aristotle described in the 'Historia Animalium.' "*Vertitur*," he says, "*in pingue quoties pabuli copia suppetit*." The muscular fibres were present in this instance nearly unchanged, but everywhere covered with fat cells, which lay in rows upon the fibre.

2d. The muscles of a paralysed youth, whose case is described by Mr Partridge (Med. Gazette, Nov. 1847, p. 944). Drawings, illustrative of the appearances shown in this case are presented. The fibres are shown in one overwhelmed with fat; in the other the fibres remain apparently very little altered, the fat having been removed by ether.

3d. The lumbar muscles of a paralysed sheep.—The preparation had long been kept in a closed bottle. (See some Account of an uncommon Appearance in the Flesh of a Sheep. By Dr Vaughan. London 1813.) The soft waxy substance was composed chiefly of fat cells, of obscure granular fibres, and of other fibres composed of delicate filamentous, but strong tissue; and, lastly, where the disease had least advanced, the muscular fibres remained.

4th. The intercostal muscles taken from between the ribs of a man.—The ribs had, for a long time, been rendered fixed by an accidental injury. I found the fat cells very abundant, and the striated muscles in many points. In other parts, the fine filamentous tissue seemed to have replaced the muscular fibres. There were also some granular fibres.

From these observations I am disposed to think, that the tendency of voluntary muscles is to degenerate into a fibrous tissue mixed with fat, rather than into granular fatty matter. This observation applies to paralysed muscles; it is not improbable, that in the flabby muscles of those suffering from scurvy and other diseases of mal-nutrition, a change similar to fatty degeneration of the heart may be found. We have, however, in the heart itself a change analogous to the fibrous degeneration just described. The tissue, then, is of a fawn colour, has a firm leathery feel, and in it we find fibrous tissue, fat cells, and muscular fibres. Dr Williams has described this fibrous degeneration. M. Rokitsky has also described this change, but he regards the fibrous tissue as unstriated muscular fibre which has been developed amongst the ordinary fibres of the heart. For further information on the fatty degeneration of voluntary muscles, consult Beclard (Anat. Générale, § 168), Mr Paget's Lectures (Med. Gazette, vol. ii, 1847), and Mr Hallett (Edinb. Med. and Surg. Journal, 1848, vol. lxi., p. 30); also Idem. 1849, vol. lxxi, p. 257.)

* Lancet, vol. i, 1842.

† Malad. du Cœur, vol. ii, p. 294.

‡ Anatomie Pathologique, 1820, tom. ii, p. 318.

§ Traité de Pathologie Interne, vol. ii, p. 337.

at these points they are seen to be composed of small crystal-line scales. The wavy outlines of the blood-vessels or nerves are sometimes seen between the fibres or fasciculi. In the intervals may be observed flat discs composed of radiating acicular crystals. These appearances vanished on putting ether on the specimen, and the merest particle of the filamentous substance remained.

From these observations I conclude, that whatever be the nature of the process, the places of the muscular fibres, the blood-vessels and nerves are occupied by fatty matter, which could not have existed in them during life. This opinion is confirmed by experiments on what may be called the artificial formation of adipocere. Mr Gibbes, whose communications to the Royal Society have been just mentioned (note, page 134), states that Lord Bacon had indicated the mode of converting the waste flesh of animals into fat, which might be used for various domestic purposes. Mr Gibbes, acting on this idea, succeeded by different processes, including exposure to running water, to nitrous acid, &c., in converting large quantities of animal flesh, even the carcase of a whole cow, into fatty matter. The difficulty which the experimenter met with in freeing this substance from colour and smell, or his death, appears to have prevented the communication of his further proceedings, which he had promised to the Society, and neither his nor Lord Bacon's discoveries have been made useful in supplying fat from waste flesh. To the pathologist they afford information of peculiar interest in connection with the following experiments. A little more than two years ago I obtained, for the purpose of examining the healthy structure of the tissue, the healthy heart of a healthy child who had died a few hours previously from the shock of a severe burn. Having satisfied myself on the required point, I placed the specimen in weak spirit and water (one part to eight or nine), for the purpose of future examination. On looking at the specimen after a few weeks, I found it greatly changed; it had a confused granular aspect, to such an extent, indeed, as to lead me to feel, that there must be some error in previous observations on the specimens of true degeneration, as portions of a healthy heart now exhibited characters so similar. I found, however, that this change existed in all parts of a heart, which I had no doubt had been healthy, in some parts at least, when previously examined. I mentioned the observation to Dr Williams, and was gratified at hearing from him, that the fact of this change was an illustration of an experiment which he had suggested to his clinical assistant, Mr Edward Palmer, sometime previously, viz. to try whether fatty matter was not formed after death by a molecular change in animal tis-

dergoing (external to the body, and thus beyond the reach of deposition, or the influence of the nutritive process), a change perfectly identical with that which it undergoes in the living body. There is, therefore, a presumption in favour of the view which holds that the processes in both cases are identical; that, in fact, when these protein compounds, albumen and fibrin, are effused in a form which is not readily susceptible of organization, when even they enter into the composition of tissues and textures, the organization of which is imperfect from inherent depravity, from natural decay, or from a deficient supply of those elements and influences, in the absence of which nutrition fails, that in these cases, I say, those substances degenerate and pass into fatty matter. In other words, that when the vital power which belongs to these higher products of animal organization is weakened or destroyed, they yield to the physical and chemical influences which surround them, and, by an inherent principle, descend into a class which is shared by them in common with plants and minerals.* It would be highly interesting to illustrate this argument by facts drawn from the multitude of instances in which different observers have recently found this fatty degeneration. Some of these facts, no doubt, readily suggest themselves, though perhaps less in the case of the heart than in other organs. It is this organ, however, which claims our attention here. An interesting observation, made by Mr Paget, supplies a most important step in this inquiry. It is now pretty well established that a cell germ, or nucleated cell, is an essential element in the nutritive process of all tissues. The first change that can be traced in the process of degeneration of the heart's fibres, and it requires care to observe it, is the destruction of these cell-germs in the tissue. Mr Paget† writes, "When the change is least marked, and but little fatty matter has collected within the sarcolemma, the outlines of the nuclei look dim, and they lose their colour. In the further advanced stage, the nucleus of the fibre cannot be seen at all; its former place is indicated, if at all, only by some out of the narrow column of yellow granules, and, in a yet later stage, or when the sarcolemma appears nearly full of fatty particles, all trace of both the nucleus and these granules is lost."

Thus we have evidence that the nutrition of the heart is impaired, and that those powers are weakened, by which it is enabled to resist the influences which tend to the disintegration of its tissue. A review of the circumstances under which this degene-

* The origin of the valuable hydrocarburet—coal from vegetable matter, suggests something very analogous, in the transformation which occurs, to the formation of adipocere from animal matter.

† Lectures, *loc. cit.*, p. 145.

ration occurs, is further confirmatory of the view adopted here. These circumstances exhibit impairment of general and local nutrition.

1. *Fatty degeneration dependent on general or constitutional causes*, is found in persons who have suffered from a variety of exhausting diseases. Amongst the cases recorded by Dr Ormerod,* some striking illustrations of this will be found. Of twenty-five cases, three had suffered from hemorrhage, three from phthisis, one from delirium tremens, one from cancer, two from fever. These cases are taken chiefly from hospital practice, and illustrate better than the cases which I have collected, and which are derived from different sources, the relation of this disease to general impairment of nutrition; still amongst my cases will be found several similar to those now mentioned. Thus, one is that of a child, who, having been long in bad health, died of cancrum oris; another is that of a young woman, who had suffered from long-continued gastric irritation and chronic phthisis; a third from hæmaturia. Several other examples, in which the nutritive functions were equally impaired, will be found amongst the cases presented with this communication, and which it is unnecessary therefore to refer to here. But besides this class of cases, there is another, in which we find degenerations going on simultaneously in other organs, for example in the walls of arteries, in the liver, in the kidneys, &c., giving evidence, likewise, of an imperfect nutritive function, acting widely, and shared in by the heart.† Of this state, illustrations, which more careful observation might have rendered more numerous, will be found in the tables. This degeneration of arteries, which Mr Gulliver has long since shown to be fatty, has already been recognised by Dr Bellingham, as being frequently associated in the form of aneurism, with this analogous condition of the heart.‡ And it is of considerable interest in connection with the second condition, viz. :

2. *Fatty degeneration connected chiefly with a local modification of nutrition*. In thirteen of the thirty-three cases of degeneration of the heart (Series I.) the coronary arteries were more or less ossified or obstructed, likewise in seven of the second series, and in five of the third.

In all the cases which I have myself examined on this point (except in a case of phthisis, and some cases allied with it), I have found more or less obstruction of these vessels. I have seen the

* Medical Gazette, vol. ii., 1849.

† The simultaneous existence of the *arcus senilis*, or fatty degeneration of the cornea, with this affection of the heart, will be subsequently referred to as an aid to the diagnosis of the latter.

‡ Dublin Medical Press, vol. xxi., 1849, p. 290.

coronary artery extremely ossified, going directly to the only part of the heart affected. (See Case 26, Series I.) A nearly similar condition will be found reported as having existed in several other cases in both series. At least, arteries proceeding to the principal seat of disease are found more or less obstructed. (See Cases 9 and 26, Series I; and Cases 2 and 3, Series II.) In the last case, the coronary artery was single and ossified. This connection between fatty softened heart and obstructed arteries suggests an analogy with softening of the brain, in which a like condition of the vessel is known to exist.*

Dr Abercrombie† has, indeed, compared this cerebral softening to dry gangrene, and he was not wrong, when the difference in the relation of the parts to external influences is remembered.

There are two causes which contribute in a marked degree to the frequent association of diseased arteries with fatty degeneration of the heart: ‡ one of these is, that the disease of the arteries is itself an evidence of the existence of a more or less impaired state of nutrition throughout the system. The other is a fact mentioned by Mr Swan,§ in a recent communication on the Blood-vessels of the Heart. It is, that there is not a free communication between the coronary arteries; that one of them cannot be readily injected from the other, hence obstructions from any cause in one, will not admit of the deficiency being compensated for by the supply from the other. Another local modification of nutrition, caused by the previous occurrence of endo- or pericarditis, is found connected with fatty degeneration. Dr Williams|| has long since observed this association; he says, "a pallid yellowish appearance of the substance due to an altered state of nutrition, is not at all an uncommon accompaniment of other lesions of the heart, such as accumulation of fat and adhesion of the pericardium connected with partial obstruction of the coronary arteries."

M. Rokitsansky goes so far on this point as to say, that this degeneration "most frequently occurs in hypertrophied hearts, in combination with the remains of endo- and pericarditis." And here it might be stated, that this author¶ believes the fatty degeneration to depend on an impairment of the nervous power of

* Since the presentation of this communication to the Society, a highly interesting observation has been published by Mr Paget, showing that fatty degeneration of the small cerebral arteries exists in many cases of cerebral apoplexy.

† Diseases of the Brain.—Edinb. 128, pp. 25 and 269.

‡ M. Bizot finds ossification of the coronary arteries more frequently in males than females, and rarely before 40 years of age. These facts correspond as to the sex in, and age at, which fatty degeneration most frequently occurs. He likewise found the left coronary artery more frequently ossified than the right, as the left ventricle is also more frequently the seat of degeneration than the right.

§ Medical Gazette, vol. xlii, p. 751.

|| The Pathology and Diagnosis of Diseases of the Chest, 1840, p. 245.

¶ Loc. cit., p. 463.

severest and most fatal lesions to which the organ is liable, viz., rupture. This lesion will be found recorded in eight cases of thirty-three in Series I., and in seventeen of thirty-five in Series II.;* or in twenty-five cases in sixty-eight. The rupture was complete, that is to say, it perforated the walls of the heart, so as to permit hæmorrhage into the pericardium in all the cases except six. One of these cases was a rupture of the septum of the ventricles; in two, there existed ruptures within the substance of the wall of the left; in one, within that of the right ventricle; in one, there was a partial rupture of the internal fibres of the right auricle; and in another, a partial rupture of some fibres on the external surface of the heart. All these cases were fatal within a short period after the accident was supposed to have occurred. The narratives of the cases suggest many points of interest in connection with this lesion, which may, on a future occasion, occupy attention. It will suffice now to say, that from the time when Harvey adduced a case of rupture of the ventricle, and the effusion of blood into the pericardium, as proofs of his then much doubted theory, this accident has attracted the notice of many pathologists, and its cause has received a variety of explanations. The actual friability of the heart appears to be quite sufficient to account for the lesion in any case in which fatty degeneration can be found; and that degeneration, I find good reason for believing, exists in the greater number, if not all, the specimens in museums which I have examined, and likewise in very many of the recorded cases of this accident, besides those from which the preceding numbers are taken. Amongst the lesions resulting from a partial rupture, are some of which claim separate mention. One of them is the appearance described by Cruveilhier,† as cardiac apoplexy. (Case 2, Series II.), and caused by hemorrhage into a portion of the heart's walls. One of the cases in the Table is a good illustration of this form of hemorrhage into the wall of the right ventricle, a situation in which M. Cruveilhier states it never occurs. (See Case 15, Series I.) An extremely interesting example of this disease has been recorded by Mr Stallard,‡ (Case 14, Series I.) The case recorded by Mr Stallard affords also an illustration of another appearance produced by this partial rupture and hemorrhage. It is when the accident has not been immediately fatal, the coagulum loses its colour, producing an appearance like an encysted abscess in the walls of the heart. These appearances

* This disproportion in these series is due to the fact, that the cases in Series I. have been taken as they occurred; whilst those in Series II. have generally been recorded in consequence of some peculiarity. Such, for example, as this very lesion.

† *Anat. Pathol.*, vol. ii., *Maladies du Cœur*, and livr. iii., pl. i.

‡ *Trans. Prov. Med. and Surg. Association*, N. S., vol. iii., 1847, p. 105.

habits of life, under which these conditions respectively occur, confirm this opinion. Lastly, in the Tables of the cases, five cases of true angina in connection with this state of heart will be found, viz. Cases 13 and 15, Series I, and Cases 2, 16, and 25, Series II, and I might add a sixth recently communicated to me by Dr Copland. Four other cases, recorded by Fothergill and Parry, will be found in Series III.

With these remarks, I conclude what I have to say on the effects of fatty degeneration of the heart, on the structure and functions of the organ. The influence of *hereditary predisposition* and the *duration of the disease*, are points still open to future investigation. I have found nothing which bears on the former subject in the recorded cases. In one of the cases recorded by myself, a sister had died suddenly in the street, it was said of diseased heart. In another case, the father of a gentleman who is living, but in whose heart I have no doubt the disease exists, died under similar circumstances.*

In regard to the duration of the disease, it is not improbable, that its progress may be rapid in some cases in which the general nutrition is greatly depraved; in other cases, however, in those particularly which occur in advancing life, it is evidently slow in its progress. Thus some cases in the table would seem to show, that it may have existed for two, five, ten, twelve, or fifteen years respectively.

V.—THE DIAGNOSIS OF FATTY DISEASES OF THE HEART.

It would be a refinement at present to seek to distinguish during life between the presence of fatty growth and fatty degeneration; I shall, therefore, allow what I have to say on the diagnosis of fatty degeneration to apply to fatty growth. I have already referred (in a note, p. 143) to the fact, that fatty degeneration of the heart is said to be frequently found after death, where its existence had not been suspected during life, and I have expressed an opinion, that such cases would not often be met with, if a full inquiry could be made into their symptoms. I feel the truth of this observation more particularly in those which occur in the progress of advancing life, when whilst the system generally keeps up its powers tolerably well, the heart suffers from some local cause, such as diseased coronary vessels. In those cases, the balance between the system and the heart is lost, and phenomena, as clear and as pointed as any that can indicate the nature and seat of disease, are developed. Again, on the other hand, in those exhausting diseases in which the heart participates, it is quite possible, that the requirements of the system may not be dispro-

* Since the presentation of this paper, a case of fatty degeneration of the heart has come under my notice, in which the brother and uncle died suddenly of diseased heart. It was said on post-mortem examination, that excess of fat on the heart was the cause of death in one of them.

PART II.

CRITICAL ANALYSIS.

ART. I.—*Traité des Maladies de Plomb ou Saturnines.* Par L. TANQUEREL DES PLANCHES, Docteur de la Faculté de Médecine de Paris. Tome Premier, pp. 550 ; Tome Second, pp. 551. Paris, 1839. 8vo.

Treatise on the Diseases arising from the Action of Lead on the Human Body. By L. TANQUEREL DES PLANCHES, Doctor of the Faculty of Medicine at Paris. Two volumes, 8vo. Volume i., pp. 550 ; Volume ii., pp. 551.

2. *Notices of the Effects of Lead upon the System.* By JAMES ALDERSON, M. A., and M. D., Physician to the Hull General Infirmary, Fellow of the Royal College of Physicians, London. Read January 8th, 1839. Transactions of the Royal Medical and Chirurgical Society, volume the twenty-second. London, 1839. 8vo. Pp. 82–94.

3. *On a Remarkable Effect upon the Human Gums, produced by the Absorption of Lead.* By HENRY BURTON, M. D., Fellow of the Royal College of Physicians, and Physician to St Thomas's Hospital. Read January 14th, 1840. Medico-Chirurgical Transactions, published by the Royal Medical and Chirurgical Society, volume the twenty-third. London, 1840. 8vo. Pp. 63.

4. *Cases of Poisoning by Lead at Claremont.* By Dr R. Gneneau de Mussy (Dublin Quarterly Journal. No. 14. May 1849.)

5. *Some Remarks on the contamination of Water by the Poison of Lead ; and its Effects on the Human Body.* By JAMES BOWER HARRISON, M. R. C. S. L. London, 1850. 12mo, pp. 32.

THE deleterious effects of lead upon the animal body are so

powerful, and act in a manner so insidiously, that it has at all times been a subject of anxious inquiry among members of the medical profession to determine both the exact effects of the mineral, the channels by which it is introduced, and the most efficacious methods of counteracting them. The metal itself is employed very extensively in the arts; and so long as it is difficult or impossible to dispense without employing it, workmen engaged either in smelting it from the ore, or afterwards preparing the metal for the different purposes to which it is applied, are all more or less liable to be affected by its poisonous operation.

Besides those workmen who are engaged in separating the lead from the ores, considerable numbers are employed in converting the metal into the various forms in which it is employed in the arts. The plumber converts it into sheet-lead of various degrees of thickness, for roofing, for lining cisterns, and similar purposes, and into leaden tubes for the conveyance of water in cities and large towns. The type-founder employs it with antimony and other metals for type metal; the house painter and the preparer of pigments employs it very extensively as the colouring matter or the drying agent in his mixtures. The potter employs it in glazing various sorts of earthenware. Various oxydes and salts of lead are employed by paper-stainers, manufacturers of German cards, and the makers of glazed cards. In short, though it has always been a metal extensively employed for different purposes in manufactures and arts, it is probably at present still more extensively used than ever; and the result is, that, notwithstanding all precautions, the number of persons who suffer in different modes from its use is very considerable.

There is reason to believe that the poisonous effects of this metal are much more frequently observed in continental countries than in Great Britain. Since the time at which Sir George Baker pointed out the hurtful consequences of preparing cyder in leaden vessels, and Dr John Hunter showed how often rum, which had been distilled by passing through a leaden worm, contained palpable quantities of this metal, a considerable degree of caution has been observed in this country in the domestic and industrial applications of lead; and the disorders which were understood to arise from the presence of this metal in cyder, perry, and similar articles, have become so much less frequent, that, from this cause, they can scarcely be said to be known as existing. Neither can it be said that diseases believed to proceed from the poisonous effects of lead introduced into the system in other modes, for instance by absorption, by inhalation, by ingestion with food, are very much on the increase.

Very different appears to be the case in France. The present author adduces a long list of trades and professions in which lead

diseases are more or less prevalent. It appears indeed from the considerable prevalence of this order of diseases, that M. Tanquerel des Planches has been led to lay before the profession the result of some very elaborate inquiries in which he had been engaged for the space of eight years. In the year 1831 he commenced a series of researches on saturnine or lead diseases; and in February 1834, he published, in his Inaugural Dissertation, the information which he had collected, and the conclusions which he had been enabled to form in one department of the subject, that, namely, of palsy, arising from the poisonous operation of lead. This treatise was looked upon with eyes of favour so encouraging, that the author was induced to prosecute the subject upon a more extensive scale, and with greater amplitude of details and illustration. He then formed the intention of preparing a work on all the diseases or hurtful effects which could be traced to the poisonous operation of lead upon the economy, the different modes in which it was introduced, and the various and variable phenomena to which its presence in the system gave rise.

This enlargement of his plan, however, rendered necessary a course of clinical observations lengthened and laborious. The Hopital de la Charité in Paris is the principal resort of the great number of workmen who are occupied in the different forms of lead manufacture; and in this Hospital M. Tanquerel des Planches found the means of conducting his observations upon an extensive scale and in a systematic manner. The physicians of this large establishment afforded the author every facility in their different departments; and for the space of five years subsequent to 1834,—eight years altogether,—the author was diligently occupied in studying upon the persons of patients the effects produced by this metal, and in endeavouring to discover the most prompt and effectual means of counteracting these effects.

Diseases arising from the poisonous effects of lead are not common unless among poor working people who necessarily apply for assistance at the hospitals. These diseases are accordingly not common in private and civil practice. The author, however, has not neglected this source of information where it was available.

In investigating the channels by which lead in different forms is introduced into the animal body with a view to the establishment of prophylactic measures, M. Tanquerel des Planches has taken frequent occasion to visit the workshops and manufactories at Paris and the neighbourhood; he has followed carefully the processes and operations of each trade; and by carefully conducting his inquiries among the workmen, the overseers, the directors, he has been able to obtain information which he thinks is more accurate and less doubtful than any hitherto adduced.

In adverting to several of the topics which M. Tanquerel des Planches has investigated, our choice must be regulated by their

comparative importance, and by the degree in which they have hitherto been imperfectly understood.

When lead reduced to a state of extremely minute division is introduced into the system, the animal economy suffers a deleterious impression more or less intense. This form of poisoning may be made known to the observer by several morbid indications well marked.

It is to be observed that the two divisions of the nervous system which preside, one over the internal or organic life, the other over the life of relation, are equally exposed to become the seat of the different disturbances which constitute Saturnine Poisoning. The mode in which these disturbances take place is not less remarkable. In the nervous system of the internal or organic life is observed only the exaltation or intensification of nervous action. In the nervous system of the life of relation, on the other hand, the phenomena of sensibility and of inability may be sometimes exalted and sometimes extinguished. Thus when lead directs its pernicious influence upon the viscera contained within the abdomen, colic in all its various forms takes place. If the spinal system be the first part attacked, there may appear in the organs of the animal life those acute pains which denote the presence of *arthralgia*, or that loss either in movement or in sensation which distinguish saturnine palsy and anaesthesia. When the brain is affected, delirium, convulsions, or coma, characterize the presence of Saturnine Enkephalopathy. In other respects lead impresses a peculiar stamp on the physiognomy of the diseases to which it gives rise.

Saturnine or lead diseases are not all equally frequent. The numbers presented by the following table, which embraces all the individual examples collected by the author, may be taken to indicate the comparative frequency of each order.

Cases of Colic,	1.217
Arthralgia,	755
Palsy,	127
Enkephalopathy	72

Among these several examples of saturnine diseases, two hundred and seventy-six (276), have not been accompanied with colic. From this statistical return of the proportions, taken in round numbers, neglecting fractions, it appears that among twenty-three individuals affected by saturnine diseases, nearly twelve persons are attacked by colic, eight are the subject of arthralgia, two of palsy, and one person only is the victim of enkephalopathy or disease of the brain.

It is merely another form of this proposition to say that colic is the most frequent form of saturnine poisoning. But very rarely does this disease appear alone; often it is complicated with arthralgia, sometimes with palsy or even enkephalopathy. In other instances each of these diseases appears separately. They may

patches of blue streaks upon the surface of the buccal mucous membrane. Lastly, in a considerable number of cases the portion of the gums next to the teeth having alone become of a slate-blue colour, presents a decided contrast to the rest of the gums, which preserve their natural rose-red hue.

The gums, before completely acquiring the slaty coloration become at first of a reddish violet tint, which eventually passes after a period variable in length, into blue.

The mucous membrane of the mouth, therefore, may present several degrees and stages of this coloration, and may be consequently stained blue through its whole extent.

This blue colour of the gums cannot be removed without great difficulty. It is only by means of frictions repeated several times in the course of the day for a considerable period, and performed with water acidulated by sulphuric or hydrochloric acid, that it is made to disappear completely.

Often the portion of the gums which becomes blue undergoes a remarkable alteration in nutrition. In some instances it is rendered extremely thin, as much so as the thinnest writing paper, or, what is still more common, it loses part of its surface. In the last case the interdental slips evidently disappear, and the concave part of the gums is removed by a process of molecular absorption, which takes place in the midst of these tissues without apparent solution of continuity.

When this process of absorption is completed the teeth are observed to be stripped of a portion of the gums; they are denuded. The gums then present only a ring, more or less prominent, which sometimes appears as if notched.

This change in the nutrition of the gums always follows the formation of the blue stain. But the latter often disappears at the same rate at which the interstitial absorption carries away the blue-stained portions of the gums. The ring also formed in this manner scarcely presents a bluish shade.

In ordinary circumstances the blue stain and the loss of substance in the gums are not observed to be equally well marked along the two alveolar margins. The anterior part of the jaws is the point at which the twofold alteration in general is most distinctly marked. The gums of the lower jaw are uniformly a little more affected than those of the upper jaw.

In some instances it happens that two or three gums only become blue coloured and undergo this loss of substance.

In a very small number of workmen M. Tanquerel des Planches observed that the gums stained blue were congested, and bled upon the slightest touch.

In one instance he found in the case of a white-lead maker an ulceration of the superior margin of the gums of both jaws. In this man the mucous membrane of the cheeks was almost every-

where stained blue. He did not suffer uneasiness. Lotions of water acidulated with sulphuric acid, which he employed in order to remove the blue staining, contributed also to heal the ulceration.

Most usually, among persons whose gums in contact with lead have acquired a slate-coloured stain, the teeth present at their base or collar, a brownish tint, very deep; while their gums present most frequently an aspect of a clearer brown, bordering upon yellow or green. This colour cannot be confounded with the ordinary tartar which covers the teeth. It is the incisors and the canine teeth which are mostly found coloured brown; but all the teeth may be so, though not all in the same degree. Where the gums have been removed by absorption, that is, by loss of substance, the brown coloration of the collar is not so well marked.

The teeth thus stained eventually become weak, brittle, are cracked, carious, and drop out before the ordinary time.

This colouring matter adheres closely to the bony substance of the teeth, with which it appears to be intimately united, and, like that of the gums, it is removed with great difficulty. It may, however, disappear under the use of the same means.

In general, when the blue stain of the gums is strongly marked, the brownish colouring of the teeth is equally distinct; and the converse.

This colouring matter of the gums and the teeth is nothing but sulphuret of lead. Oxygenated water placed in contact with the gums and the teeth so stained gives rise to a whitish streak, which is sulphate of lead. M. Tanquerel des Planches digested in hydro-sulphuric acid the gums and teeth of a colour-grinder, who had died in consequence of saturnine disease in the brain; (*enkephalopathia*). At the end of twenty-four hours, a great part of the gums, which was not visibly covered by this bluish staining, became of a deep blue colour.

Of the mode in which this substance is formed on the gums and teeth of persons working among lead preparations, the author gives the following explanation. Alimentary articles comminuted by mastication leave some of their fragments in the mouth or interposed between the teeth. Amidst these remains of alimentary articles, which always contain a little sulphur, and are speedily decomposed, sulphuretted hydrogen gas is formed. This gas and the minute particles of lead which pass through the mouths of individuals who respire or swallow such particles, are placed in mutual contact; and in consequence of chemical affinities, these substances are decomposed, and sulphuret of lead is consequently formed, and deposited upon the gums and the teeth in those very spots where the sulphuretted hydrogen is produced.

If in other parts of the economy besides the mouth, sulphuretted hydrogen is formed in the uncombined state, undoubtedly

we should ascertain in these parts the presence of sulphuret of lead in instances of saturnine impregnation; that is, when the lead particles would come into contact with this gas. In the whole organized body sulphur enters as an element of several of the solids and liquids; but it is intimately united with other constituent substances of animal matter; and this union forms an obstacle to disaggregation, and consequently to its conversion into free sulphuretted hydrogen capable of being combined with the lead.

Saliva, for example, in the composition of which there is a considerable amount of sulphur, is not decomposed in the presence of lead. Its sulphur does not combine with the particles of this metal in sufficient number to hold the lead in solution or simply in mixture, in persons in the early stage of lead impregnation. M. Tanquerel des Planches examined this saliva by the microscope, and also chemically. Nothing abnormal was found.

The loss of substance in the gums by interstitial absorption or molecular atrophy, their congestion and ulceration, as well as caries and destruction of the teeth, are probably explained by the obstruction of the capillary vessels arising from the accumulation of sulphuret of lead. This renders nutrition impossible.

After from five to six days, months, or whole years of exposure to the contact of lead particles, we observe the brown staining of the teeth and the slaty staining of the mucous membrane of the mouth to be established.

A great number of white lead makers present marks of this staining at the end of some days of working; house-painters are usually attacked only at the end of some months of the exercise of their trade; while among crystal cutters and polishers the indications of this formation usually do not take place until after several years of working.

The colour of the gums and teeth now described M. Tanquerel des Planches never observed except among individuals in whom the mucous membrane of the mouth is in contact with particles of lead. He examined the mouth in seven hundred and eighty-five persons, who had not worked among lead preparations, or who had not swallowed the metal in any manner; in not one of these did he discover the slightest trace of the specific staining produced by sulphuret of lead.

This stain, therefore, he infers, forms an excellent means for recognizing whether any individual has swallowed or respired particles of lead. It is in truth a diagnostic mark of the primary action of lead upon the economy.

Nor in all persons who respire or swallow particles of lead for some time, are the teeth and gums covered with sulphuret of

these patients could give a cause for his illness, yet the supposition that both of them were under the influence of lead is quite consistent with the facts related in the numerous histories published of its action on man, and which, in the cases of the two men under consideration, suggest the extreme probability that they had been unwittingly exposed to the action of the metal, derived from one or more of those many sources which have been elaborately pointed out by Sir George Baker, and subsequently by Dr Christison."—Pp. 66–72.

From various facts recorded by different authors, and from personal experience, Dr Burton expresses strongly the belief that the introduction of lead into the human body is continually taking place, in an unobserved manner, to a much greater extent than is usually supposed, and that it has often caused an ambiguous assemblage of morbid symptoms. This is very likely to be the case; and the fact was well illustrated in the instance of the ex-royal family of France at Claremont in 1848.

From all the facts, in short, which had come under his notice, Dr Burton was led to form an inference very similar to those given by M. Tanquerel des Planches, namely, that the blue line upon the gums may serve not only as an excellent and sure diagnostic mark of the presence of lead in the system, but that, as this takes place in general previous to the formation of the symptoms constituting lead colic, it may be taken as a means to warn the physician in administering lead, when he should stop the exhibition of the remedy. In reference to this subject, among twenty-seven patients treated by Dr Burton during the course of a few years, by means of acetate of lead and opium, in at least twenty persons no colic symptoms and no material inconvenience were induced except constipation. In two or three cases the colic symptoms were severe; but in these hemorrhage was profuse, and the dose was large. With ordinary precautions, however, colic does not occur in a severe form during the administration of lead.

From a table of eleven instances given by Dr Burton, it appears that the shortest time for the production of the blue line upon the gums is four days; and that the smallest quantity under which it was produced was fifteen grains of sugar of lead. In two cases it took seven days; in the others it took ten, eleven, twelve, fourteen, and twenty-one days. In the case last mentioned, 160 grains of sugar of lead were taken to produce the blue line; the patient had no colic; but death took place in consequence of hemorrhage from the lungs. Dr Burton further states that Mr Moyle of Chacewater produced the discoloration in the course of twenty-four hours, by giving four doses of five grains each every six hours.

The question has been stated more than once, whether the

deserving to be known had escaped his notice. Upon this part of the subject, therefore, it is quite unnecessary to enter.

The consideration of the modes in which the poison may be introduced into the animal body is a matter both of interest and practical importance.

Saturnine preparations, before entering the system so as to affect the actions of the body, must undergo a previous solution in a liquid, or must be reduced to a state of very minute division, or must be presented in the form of vapour.

All saturnine preparations, metallic lead is of course excepted, are more or less soluble in a great number of fluids of the mineral, vegetable, and animal kingdoms. They are easily reduced into fine dust or powder, which, by reason of its lightness, may be disseminated in the atmosphere, particularly by the aid of currents of air. The saturnine chemical compounds, that is the oxides and salts, are not of themselves very susceptible of being converted into vapour in the open air. Metallic lead appears to be the only article capable of undergoing in a slight degree this transformation. But the former may be volatilized, that is, they may be detached in the form of a thin vapour when they are exposed in certain circumstances to currents of gas or of vapour. In certain circumstances the dispersion of saturnine particles is effected by the aid of a volatile vehicle,—not by themselves.

The author allows that the terms *volatile* and *volatilized* are not in this instance very accurately employed. All that is meant is that the saturnine particles are put in that mechanical condition in which they are carried by the atmosphere or by similar vapours to the surfaces of the animal body.

The saturnine particles, whether solid or gaseous, in contact with the membranes of absorption, must, as all other substances foreign to lead, undergo previous solution by the organic liquids in order to be absorbed. It is only in consequence of this absorption that particles of lead can give rise to saturnine colic; and this circumstance it is, which forms a decided difference between this disease and the accident which constitutes inflammatory poisoning by the introduction of poisons of lead into the stomach. The former operates through an indirect, the latter through a direct action. The former operates by minute quantities gradually and successively accumulated; the latter produces its effect at once by the largeness of its amount and the speediness and energy of its action. In the last case the poison is not absorbed, and does not require to be absorbed. It irritates the mucous membrane in contact with which it is placed, and may cause in it inflammation and destroy it. This accident, consequently, which belongs to the head of poisoning by irritant or corrosive substances, requires not to be considered in this place.

Lead and all its compounds appear to be capable of giving rise to lead colic. At least all the saturnine preparations which hitherto have been placed in contact with the organs of the human body are endowed with this faculty.

The following list of these substances is given.

METALLIC LEAD.

OXIDES OF LEAD.

Suboxide; (the oxide of Berzelius). The *pulverulent protoxide*; (massicot). The melted *protoxide*; (Litharge). *Protoxide* and *Binoxide*; (minium).

SALTS OF LEAD.

Borate of Lead.

Subcarbonate of Lead (Ceruse; White Lead; White Silver; White of Krems; Venice White; Hamburgh White); (Dutch White), *Neutral carbonate of Lead.*

Phosphate of Lead.

Chromate of Lead; (Mineral Yellow; Paris Yellow; Verona Yellow; Turner's Yellow; Kassler's Yellow; Chrome Yellow; Siberian Red Lead).

Azotate of Lead; (Nitrate of Lead).

Acetate of Lead; (Neutral Acetate of Lead).

Sal Saturni; (Sugar of Lead).

Subacetate of Lead; (*Extractum Saturni*; vegeto-mineral water; extract of Goulard).

Hydrocyanate of Lead; (Cyanuret of Lead).

Silicate of Lead.

Sulphuret of Lead (Black Lead; not the carburet of iron).

Sulphurous Lead Ore; (Alquifoux).

Chloride of Lead; (horn-lead; hydrochlorate of lead).

ALLOYS OF LEAD.

Alloys of Lead and Tin; (Plumber's Solder).

Alloy of Lead and Antimony; of *Lead and Copper*; of *Lead and Silver*; (argentiferous lead ore); *Alloy of Lead and Gold.*

Other saturnine compounds, which are puce-coloured oxide, the molybdate of lead, iodate of lead, seleniuret of lead, arseniuret of lead, alloy of lead with iron, plumbites of potass, of soda, of baryta, of strontia, and of lime, oxalate of lead,—being only products artificially procured and without use, the human race are not often placed in circumstances such that they can absorb these articles, and consequently are not attacked by colic from their introduction into the system. If, however, these substances were extensively distributed in the natural world, or worked and manufactured for the purposes of social life, it cannot be doubted that they are capable of giving rise to this disease.

entire could produce in animals the symptoms of saturnine poisoning. With this view he rubbed over the skin of dogs strong ceruse ointment; he applied the diapalmic plaster secured by diachylon plaster, and the Mother ointment over the shaved skin; on a rabbit he applied strong litharge ointment, all in such a manner as to show by chemical tests that the lead was well applied. In no instance, nevertheless, were any symptoms of saturnine poisoning produced.

M. Tanquerel accordingly concludes that the statements made by many authors regarding the cutaneous absorption of lead, while the epidermis is entire, cannot be credited. The epidermis in truth forms an obstacle either quite unsurmountable or not easily penetrable by any of the common salts of lead.

It must not be forgotten, nevertheless, that Orfila, Dr. Pereira, and other good authorities, regard as inconclusive these arguments regarding the non-absorption of lead through the skin, while the epidermis is entire. Dr Alfred Taylor,* who refers to various known instances of lead poisoning by external application, speaks of it as a point not to be questioned. He states, that even the pure metal frequently handled may thus find its way into the system, unless strict cleanliness be observed. This we understand to apply to the condition of the epidermis being retained in a state of integrity. The statement made by M. Tanquerel, that actors who employ carbonate of lead as a pigment, to give the pale colour to the countenance, are not attacked by colic, is contradicted by other testimony. But independent of this, it seems difficult, without admitting cutaneous and epidermal absorption, to account for the bad effects which, wearing Brussel's lace, occasionally produces upon some persons. This manufacture is whitened by means of carbonate of lead; and serious bad symptoms are said to have, in certain instances, followed in those who wear it. In short, at present it must be observed, that the inferences of M. Tanquerel, regarding the absolute protecting and resisting power of the epidermis, must be received with limitation.

On the other hand, when the epidermis is removed, and consequently in open sores and excoriated surfaces, the absorption of lead is known frequently to take place, though not always to such extent as to produce the peculiar symptoms of lead colic.

By the mucous surfaces absorption is produced with little difficulty. Indications of such absorption have been observed to take place by the ophthalmic mucous membrane; by the vaginal mucous membrane, and the extensive membrane of the alimentary canal.

Medicines prepared from lead, especially various preparations of sugar of lead, are, if not the most usual and direct modes by which lead is introduced, at least not uncommon means. M. Tan-

* Taylor's Toxicology, p. 448.

querel, after referring to various recorded examples of this incident, expresses the opinion, that saturnine preparations introduced as medicines occasion colic at present, more frequently than is usually believed. He states that he has seen these substances administered only four or five times, and twice has he seen colic and other saturnine diseases induced. He thinks that when intestinal symptoms take place under the use of these agents, they are either overlooked or are ascribed to the original disease. This, we think, is not very likely in this country. But at all events it may be not improper to refer M. Tanquerel to the memoir of the late Dr Burton upon this point.

Lead is liable to be introduced into the alimentary canal in articles of food and drink. Gaubius affirms that in Belgium some unprincipled dealers mingle white-lead with the butter, in order to increase the weight of the butter, and to give it a white colour, which it appears in that country and at that time was highly prized. In consequence of this adulteration the physician saw several instances of lead colic produced.

Most kitchen utensils and vessels, especially those of copper, are lined by an alloy or tinning, which consists of tin in large quantity and a small proportion of lead. When this tinning is new and well done it causes no inconvenience. But when it is partly worn, in consequence of long-continued use, some portions of this alloy may be mixed with the food prepared in these vessels, and may thus become the cause of severe accidents. Thus it is reported by Barruel, that a pudding baked in a copper pan or shape, the tinning of which was worn, produced severe symptoms of abdominal poisoning, owing to some particles of oxide of lead which it contained, and which were derived from this tinning.* The lead probably did mischief; but it may be asked, in this case, what was the state of the inner surface of the pan?

This objection seems to have occurred to the author; for he admits that sometimes poisonings produced by utensils with worn tinnings are not caused by saturnine colic. These, he allows, are attacks of gastritis and enteritis induced by the introduction of a considerable quantity of lead into the stomach, or the mixture of some parcels of lead conjoined with other poisonous substances, for instance the oxides of copper.

The author takes a view of the noted subject of the sophistication of eager sharp wines by means of lead. But neither in this nor in his facts upon the contamination of water by means of lead is there any thing new or peculiar.

The author could not probably be aware of the facts as to the action of wine upon lead brought to light by Dr Alfred Taylor; for if we mistake not, these facts were made known subsequent to the appearance of the work of M. Tanquerel. Dr Taylor, it may be

* *Annales d'Hygiene et de Medecine Legale.* Tome x.

lead is possessed in the strongest degree by sulphates and carbonates, in a slighter degree by muriates. Thus carbonate of lime or carbonate of soda may equally render water incapable of being acted on by metallic lead; but muriate of soda and muriate of magnesia are less useful in this way, and allow lead salts to be formed. A mixture of equal parts of river water and distilled water exerted no action on lead; consequently, infers Dr Taylor, a preservative power is exerted where the proportion of saline matter could not have been greater than one-fourteenth thousandth part. The Edinburgh water, which contains, according to Dr Christison, about one-twelve thousandth part of saline matter, has but a feeble action upon metallic lead. The same authority thinks it a just and safe conclusion to establish, that water which contains less than an eight thousandth part of salt in solution, cannot be safely conducted in leaden tubes without adopting certain precautions to prevent chemical action. This proportion will prove insufficient to prevent chemical action, unless a considerable part of the saline matter consist of carbonates and sulphates, especially the former. When the salts in solution are in a great measure muriates, a proportion so large as a four thousandth part, or even more, will be insufficient to prevent all mutual action.

The knowledge of these facts has led to various useful inferences regarding the circumstances in which lead, which is conveyed in leaden tubes, and retained in leaden cisterns, communicates to the water poisonous properties. When leaden tubes are newly laid, and when water is received in a cistern lined with new lead, it often happens that the hydrated oxyde and carbonate of lead are necessarily formed at first; and corresponding to this formation, it has been known that accidents of saturnine poisoning have resulted. These accidents, however, although they take place during the first employment of the tubes and cistern, gradually cease, and do not again recur. The reason, it is said, that they do so, is because, after the first formation of carbonate of lead, the lead ceases to undergo any further change, and no more carbonate is formed to be removed by the water. It is conceived, therefore, to be injurious to clear away from the interior of lead tubes or cisterns the incrustation by which they are lined.

A source of contamination of water in cisterns was first observed by Dr Paris, and has been admitted by Dr Pereira and Dr Taylor. When the cover of a cistern is lined with lead, the water which rises by evaporation, being pure and free from saline matter, exactly like distilled water, acts with certainty and expedition upon the lead lining, and forms the hydrated oxide and carbonate, which, again dropping into the water below, may sufficiently impregnate that to render it poisonous. The rule to be observed

craft. From this it follows, that when these persons, the most numerous class of the patients who apply at La Charité, come to the hospital labouring under lead colic, in almost all cases they give their names for inscription in the Hospital Register, with the designation of their original profession; for instance, mason, plasterer, earth-worker, distiller, mariner, soldier, and so forth.

In the returns to be made from facts thus collected, it would be wrong to place to the charge of one of these original occupations a disease which it could not have caused. This, however, M. Tanquerel argues is what has been actually done by trusting implicitly to these registers, in so far as M. Merat and various others have thought, that lead colic might proceed from the effect of many other substances besides lead.

It is enough to say M. Tanquerel des Planches has been aware of this source of error, and has avoided it.

A difficulty of a different kind though not unallied to this has presented itself in these etiological investigations.

A female was received into the Hospital de La Charité under M. Rayer, labouring under symptoms which bore a perfect and exact resemblance to those of lead colic. This female, however, was a portress, and asserted that she had taken no medicine for from five to six years, that she had never dwelled in newly-painted rooms, and had in no manner worked in lead. In short, all the questions proposed were unable to prove that the woman had been in contact with lead. The fact appeared to M. Tanquerel so extraordinary, that he resolved to employ all means in order to discover the cause of the disease. He repaired to the abode of the patient in the *Rue des Petits-Augustins*. She occupied an upper room, the floor of which was in very bad condition, and was perforated by many holes. Immediately below this floor in the ground-flat was the laboratory or workshop of a perfumer. In this place were pounded and sifted, at the beginning of every month, several pounds of white lead, in order to be incorporated with several cosmetics, pomatums, &c. During this operation part of the air of the laboratory, loaded with particles of white lead, readily passed through the fissures and holes of the floor of the room inhabited by this female.

It was therefore not surprising that this woman should be attacked by lead colic. The gums and teeth of this patient presented in a small extent the blue line, denoting sulphuret of lead. Had this case not been made the subject of careful inquiry, it might have been supposed that the colic proceeded from some other cause.

1. *Workmen in White Lead Manufactories.*

The earliest manufactories of white lead, or subcarbonate of

lead, according to the author, were established in France sixty years ago about the year 1791, by M. Chaillot of Paris. From that time lead colic became a disease greatly more frequent than formerly.

The most noted manufactories of white lead established in France are those of Clichy and Courbevoie near Paris, of Lille, Strasbourg, Orleans, Lyons, Dunkirk, Marseilles, Pezenas, and Nantes. The number of persons engaged in these works arises at least to five hundred,—men, women, and children. In Germany, Holland, and England are also a considerable number of manufactories of white lead.

In all the white lead manufactories of Europe there is prepared an enormous quantity of subcarbonate of lead; because the substance is extensively employed in the arts, because it forms the basis of almost all the mixtures used in oil-painting.

In 1831, that is, at the time at which M. Tanquerel des Planches commenced his researches upon saturnine diseases, there were in the Department of the Seine, and the Seine-and-Oise, five manufactories of White Lead. The workmen who were taken ill were sent to the hospitals of Paris, and especially to that of La Charité.

	Workmen.
Clichy employed - - -	50
The two manufactories of Le Pecq, -	30 to 35
Courbevoie, - - -	20
Port-a-l'Anglais or Vaugirard, -	6
In all, -	<hr/> 106

In 1839 the two manufactories of Le Pecq, as well as those of Port-a-l'Anglais and Vaugirard, had ceased from active operations. Hence at and some time previous to this period, 1839, patients came only from Courbevoie and Clichy.

The works, nevertheless, are not in activity the whole year round. For whole months only a few workmen are employed in the mere commercial department only.

Notwithstanding this small amount of active occupation, the class of white-lead makers at Paris, though not numerous, furnish an amount of cases of lead colic equal to one-third of that of the total population of all the states who employ lead, however extensive it be elsewhere. The cause of this great prevalence of the disease M. Tanquerel finds in the nature of white lead and the processes of manufacture.

Subcarbonate of lead cannot be converted into vapour. But it is easily reduced into a very fine powder or dust by mere handling. Friction, percussion, scraping, braying, in one word, all those operations, the object of which is to break and dissolve the aggregation

of its particles, have the effect of dividing it into particles so light, that they are disseminated with the greatest facility through the atmosphere.

The most usual processes employed for the manufacture of white lead are the following ; the Dutch process ; the French process ; the process of Reboul or the Austrian method ; and the process of Torassa and Walkerwood.

The Dutch method, which is the oldest known, consists of the following series of operations.

First, the lead is melted and formed into thin plates.

Then after spreading a layer of manure, in some places tanners' bark is used, over this conical earthen vessels containing vinegar are placed in rows, and on each vessel a thin plate of lead. Over this is placed a sort of floor or covering of deal. Above this is spread another layer of manure, with rows of earthen vessels containing vinegar, and each covered by a thin plate of lead. Successive layers of this construction are formed, until the chamber in which this operation is arranged, is sufficiently filled.

It is understood that the lead is first converted into acetate. But while this is proceeding, the fermentation of the manure gives rise to the formation of carbonic acid, which unites with the lead and forms a subcarbonate.

At the end of about one month, as soon as the surface of the lead has been well converted into subcarbonate, this subcarbonate is removed from the metal by striking the plates with a wooden club.

This beating process is sometimes insufficient to detach all the subcarbonate formed. The men are obliged to pick off these adherent portions with their hands. This operation, in short, is known among the men by the name of picking.

The residual lead plates not carbonated are again melted to be used in the same manner. In some instances, however, especially where tanners' refuse is employed, and the lead is exposed a sufficient time, the whole is thoroughly converted into subcarbonate.

The carbonate thus detached is put through the mill, then bouted, and finally sifted.

The white lead is then put up in small casks and is ready for the market.

When it is wished to prepare white lead, which is esteemed very fine, the finest part is selected and diluted with water. The paste thus formed is subjected to another grinding between horizontal millstones.

The white lead, still moist, pasty, but sufficiently trituated, is put in pots which are placed in a stove to be dried. This lead is then wrapped in paper, and is ready for the market.

This is the method pursued at Courbevoie.

In all these operations a considerable disengagement of saturnine dust or fine powder takes place. Some of the operations are more dangerous than the others. Thus the beating and picking are both dangerous, because dust is produced by the operation. But these are as nothing compared to the grinding, the boulding, the sifting; and, in the making of very fine lead, the second milling and the potting. The second milling process, which is done with the aid of water, M. Tanquerel regards as less dangerous than the others. The fact that white lead in powder is detached and diffused through the atmosphere has been also proved experimentally. This was scarcely requisite; for simple observation is sufficient to show that the dust is readily carried in this manner through the atmosphere of the places in which these operations are conducted.

Almost the whole surface of the bodies of the workmen is covered with particles of white lead. It may be divided into a white dust so fine, that it is not perceptible by the naked eye in the cutaneous furrows to which it adheres. But if these points be touched by sulphurous water, the presence of lead is instantly indicated by the black stain. Some of these men even who have as a matter of cleanliness washed the surface of their persons carefully with soap and water have apparently cleared away all traces of lead; but, upon bathing in a sulphurous bath, the true state of the skin was shown by the dark colour which it speedily assumed. This dark tint was most conspicuous and distinct at the nails, at the forepart of the thighs, at the level of upper end of the breastbone, on the chest, over the navel, and at the palmar surface of the hands.

In the French method, which is practised at Clichy, litharge, or the protoxide of lead, is exposed to impure vinegar, and through the watery solution of acetate of lead thus formed a current of carbonic acid gas will be transmitted.

In this way the carbonate of lead is obtained in a moist state, and retaining still a little undecomposed acetate of lead. In this condition it is carried by means of a sucking pump into large troughs, where it is thoroughly stirred and washed, in consequence of which the acetate is washed away through channels made in the walls of the troughs, while the ceruse is left behind in the solid state.

The ceruse is then filtered.

After this it is placed, still in the form of a moist soft paste, in pots where it is exposed to a gentle heat in a stove. In this state it is in the form of loaves ready for the market.

If it is wished to have it in the form of powder it is passed through the mill, then through the sieve, and then put up in casks.

Each manufacturer affirms, without hesitation or comparative view, that his own method is the most safe; and physicians not following the workmen in the subordinate details of their labours can do little more than receive and assent to the representation of the overseer or director. The author repeatedly visited the works of Clichy and Courbevoie, and interrogated the workmen who had left these works as patients, with the purpose of ascertaining the relative numbers of men where white lead was made according to the French and Dutch methods respectively. These inquiries have furnished the following results.

Among the four hundred and six white-lead makers specified in the table, two hundred and ninety-four had worked at Clichy, eighty-eight at Courbevoie,* fifty at Pecq, and twenty at Port-a-l'Anglais or at Vaugirard. It must be remembered that Clichy employs nearly three times the number of workmen more than Courbevoie, so that it is not wonderful that this last manufactory has sent less than one-third of the patients. From the other works in which the operations were continued not more than two or three years, no accurate calculation can be formed.

The white lead makers, who work according to the French or the Dutch method, contract lead colic after continuing at these establishments for a space varying from three days to six years.

Of the workmen observed by the author, those coming from Clichy have worked on an average term fifty-one days before being attacked by colic; those from Courbevoie have ceased on an average after fifty-seven days' work. This difference in the average number of days, which a workman may be exposed in each of these establishments, the author thinks is sufficient to justify the inference, that in one of these manufactories the men are more speedily affected than in the other. He adds, however, that no positive conclusion can be drawn as to the greater hurtfulness of the French method than the Dutch; for he observes there are, besides the modes of manufacture, other circumstances which render the danger relative.

For example, he continues, at Clichy, twenty times more ceruse is made than at Courbevoie, and consequently he thinks the workmen should more rapidly be taken ill.

Further, at Clichy all the processes are conducted in the same

* There is some error in these numbers, but we cannot venture to correct it. Taken altogether, they make not 406 but 452. Thus—

Clichy,	...	294
Courbevoie,	...	88
Pecq,	...	50
Port-a-l'Anglais, &c.,	...	20
		452

The number 294, which most probably is not right, would require to be 248 in order to give the sum 406.

house; all the workmen are in contact with the emanations issuing from the whole body of white lead which is manufacturing in one single workshop. On the other hand, at Courbevoie, the processes are conducted in separate apartments, and consequently they are not exposed to the contact of the whole dust of the establishment. This difference in arrangement, M. Tanquerel thinks, ought to exert a considerable influence upon the health of the workmen.

The general correctness of these inferences, M. Tanquerel thinks, is proved by the fact, that for one year previous to 1839, at Clichy, a small quantity of ceruse was made according to the Dutch method, in the same workshop where the ceruse was manufactured by the French method. M. Tanquerel remarked that the workmen were always attacked at a more early period than those of Courbevoie. He thinks it further probable that the ceruse of Clichy, obtained as it is by the French method, being lighter than that of Courbevoie from its containing sugar of lead in large amount, may be more easily disseminated through the air, and hence may more readily affect the workmen. It has been also observed that in the different trades, as house painters and others, who make use of white lead in large amount, the white lead of Clichy occasions more frequently and more easily lead colic, than that which has been prepared according to the Dutch method.

If the statements of M. Reboul, the proprietor of the manufactory at Pezenas, on the other hand, can be believed, lead colic is a greatly less frequent disorder there than either at Clichy or at Courbevoie. Though M. Tanquerel thinks that this gentleman's assertions as to the unfrequency of colic at Pezenas are not free from exaggeration, he allows that the workmen are there less exposed to the injurious operation of the white lead than elsewhere. He observes, that as the white lead, when withdrawn from the stoves, is in the moist pasty condition, and does not require to be brayed, but merely crushed, dried, and put in packets, there is much less room for the inhalation, deglutition, and other modes of introduction of the poison, than where the white lead is ground or brayed, bouted, sifted, and passes through all the states which a fine dry powder must pass through.

In conclusion, it is an irresistible inference, that among all the workmen who have occasion to handle saturnine preparations, the white lead makers are those who are most frequently affected by lead colic. The managers, superintendents, and overseers of these establishments are themselves attacked by the disease. In no other profession are men so incessantly and thoroughly surrounded on all sides by so large and fully impregnated an atmosphere of saturnine emanations. These persons breathe, so to speak, lead, and they may swallow it every moment.

behind is taken out and again placed in the oven for another calcination. An hour and quarter, or one hour and a half, are usually sufficient for one washing operation.

When the workmen take care not to agitate the massicot in placing it in the tun, they do not disseminate the saturnine dust during this operation. But their hands are often in the washing tun, where they come in constant contact with the massicot.

After the massicot has been duly brayed and washed, it is conveyed to the drier or kiln, situate above the calcining oven. This drying kiln is surrounded with a wooden parapet seven or eight inches high. The liquid massicot poured into this receptacle is at the end of two or three days sufficiently dry. During the desiccation a thick vapour or smoke, consisting of numerous particles of moist massicot, arises from the kiln. The workman, when he removes the massicot from this place before it is completely dry, is in the midst of an atmosphere saturated with leaden particles.

The massicot removed by means of shovels is placed in cisterns of beat iron for a second burning or calcination, which receives the particular designation of *reverberation*. With this intention the workman arranges the cisterns in such a manner that they are piled on each other to the ceiling. All access to the external air is excluded as much as possible. For this purpose the workman closes the register of the chimney, the mouth of the oven, the doors of the ashpit, and the fire-place. Hitherto the calciner is very little exposed to the contact of particles of oxide of lead. But next day, on opening the oven, a great quantity of saturnine dust is disengaged in consequence of the currents of air which rush on all sides into the oven.

The cisterns, when removed, contain a product of a colour already intensely red. This, which is called *one fire red lead*, is a mixture of protoxide and deutoxide. It is then reduced to powder in a boulting machine, consisting of two cylinders, one placed above the other. Though this braying machine is surrounded by a wooden case, a great quantity of dust escapes and surrounds the workman.

Thus pulverised dry, the *one fire red lead* is subjected to another reverberation in the iron cisterns in the oven as at first. This roasting converts it into *two fire red lead*, or the red lead of commerce.

The final operation is the barrelling, perhaps the most dangerous of all. It is easily understood.

There is another species of red lead, or oxide of lead, known in commerce under the name of *orange mine*, or *English mine*. This is ceruse or white lead reverberated two or three times after the manner of massicot.

The description now given will enable readers to understand

House painters are in various modes, besides the act of mere painting, exposed to lead emanations. The practice of burning off old paint from walls and doors, which M. Tanquerel says is not unfrequent, and which is done by covering the surface with oil of turpentine and then setting fire to it, is mentioned as a service in which lead emanations are abundantly detached.

It is said, on the other hand, that the use of spirit varnish, and what is called fat varnish, with or over oil painting, protects the artists in a great degree from the hurtful effects of lead. Painting in size seems to cause little danger, though white lead forms a considerable part of the composition.

The colour-grinders, whether as a separate profession, or as dependent on house painters, are frequent sufferers.

5. *Coach painters* are not very frequently attacked by colic. Of these men about one thousand were in Paris in 1839. The number in the table, 47, is about one in twenty-one.

6. *Sign painters, letter painters, and fancy painters* are said all to be much less frequently affected by colic than house painters. These workmen paint on grounds previously prepared by the house painters; but they use infinitely less lead in their operations than house painters. In other respects they perform on the small scale what is done by house painters on the large.

7. *Porcelain painters* are a species of decoration or fancy painters. They employ oil of turpentine, fixed oil, red lead, white lead, and chromate of lead, for painting on vases and other porcelain articles. But the saturnine preparations which they use are in small quantity, and they handle them little. They are not very subject to the disease.

8. *Portrait painters* are not at the present time, and have not for a long time past been very much affected by lead colic. The substances are prepared before hand by others. These artists do not use oil of turpentine. Formerly, it is supposed that they were liable to the disease. A report has been occasionally prevalent that the early death of Corregio and of Raphaelle, as well as other celebrated painters, was caused by lead poisoning, and Tronchin has favoured this report. M. Tanquerel differs and entertains doubts. Of the mode of the death of Corregio we have at present no exact information. But Mr Dennistoun,* in his recent interesting work on the Dukes of Urbino, gives on the death of Raphaelle sufficiently detailed and exact information, to show that poisoning by lead was out of the question. The professional labours and incessant duties of Raphaelle had overtaxed the powers both of mind and body. "A frame naturally delicate, exhausted by mental fatigue, was incapable," he tells us, "of resisting the first attack of

* *Memoirs of the Dukes of Urbino from 1440 to 1680.* By James Dennistoun of Dennistoun. Vol. ii. book iv. chap. xxix., p. 230. 3 vols. 8vo. London, 1851.

stone, and over which the workman carries in all directions, in circles and ellipses, a sort of heavy pestle or milling stone, shaped like a truncated cone, the larger base being the one moved over the table, and its surface being slightly convex. The workman is much exposed in this operation to saturnine dust and vapours; yet this is not the worst. Previously to the grinding process he has often to break the loaves of white lead and chrome yellow by a mallet or hammer. This is the most dangerous part of the trade.

Grinders who grind ceruse and chrome-yellow with fixed oil or cold water, after these substances have been coarsely crushed in the dry state, do not, it is said, contract colic. In this condition are grinders of colours for porcelain painters.

Various ingenious mechanical contrivances have been devised to diminish and obviate the dangers to which these men are exposed. Thus mills and grinding machines, enclosed within cases of wood or tinned iron, so as to prevent the escape and diffusion of dust, have been proposed and employed. The result of all these means, and perhaps of others analogous, is that the colour grinders are now much less frequently attacked by lead diseases than formerly.

14. *German and Glazed Card Makers.* The German card makers employ, for putting on their colours, white lead, red lead and litharge, with other colouring substances, suspended in a solution of glue and water. In order to apply the mixture, it must be boiling; and in this temperature it is said much lead is diffused through their work-shops.

Among twenty-four German card makers, only fifteen have not been attacked by the disease. This is above thirty-seven per cent.

15. The *glazed card makers* are also very liable to be attacked by colic. The material is white lead, suspended in size. It appears, however, that in the large glazed card manufactory of M. Hangrand, in the Rue Meslay, it is chiefly the men employed in braying the white lead who suffer by colic.

16. *Perfumers.*—These persons, from using white lead in various cosmetics and requiring previously to reduce it to very fine powder, are not unfrequently attacked. It appears to be chiefly when they are melting the lard and mixing this and the oils with the lead, at a high temperature, that attacks are produced. Handling these pomatums cold produces no inconvenience or danger.

17. *Belt-makers.*—The *belt-makers* whiten with ceruse the belts of sabres and numerous other leather accoutrements. The white lead formed with water into a paste is spread by a pencil or brush over the leather. When this covering has had time to dry the workmen rub and brush it forcibly, an operation which causes the dispersion of a certain amount of saturnine dust. They also previously bray the white lead to a sufficient degree of minuteness. Both are dangerous operations.

and give to glass thus formed a weight, softness, and a degree of cleanness which no other substance can communicate. Coloured glass, crystals, and glasses for optical purposes, are those which contain the largest quantity of lead. The optical glasses of Faraday contain borate of lead. Silicate of lead performs an important part in the manufacture of crystal, of strass, and other dense glasses for optical purposes.

Workmen engaged in this manufacture are nevertheless soon after, and in great numbers, sufferers by the disease.

Enamel-workers.—These are very little affected.

21. *Stokers and Engineers in Steam-vessels.*—Many of the firemen and enginemen in the steam-vessels sailing from Havre have suffered from the lead colic. According to Duchemin, the cause is that these men employ a mastic or putty consisting of carbonate and deutoxide of lead.

22. *Lead-mine workers.*—France has several lead mines. The most celebrated is that of the Poullaouen in Brittany.

Lead occurs either as a sulphuret (galena, lead-glance), which is the most common, or as a neutral carbonate, as at Poullaouen, or as a phosphate. It is the burning and smelting of these ores that prove dangerous occupations to the workmen. This and the previous crushing and grinding of the ores often give rise to lead colic.

23. *Plumbers.*—These workmen are often attacked by the disease in Paris. In this country they seem to be a healthy class, and are not often the subjects of lead colic.

On *Tinsmiths, Pewterers, and Tinsolderers* nothing peculiar is communicated. Though all are exposed, they do not often contract the disease.

24. *Type Founders.*—These workmen are distinguished in Paris into two orders. One consists of the *founders*, properly so called, by which is meant the melters and preparers of the type-metal. The other is the order of type casters. Both are exposed to lead emanations; but the former in a greater degree than the latter. The latter, it is said, have the habit of preparing their food and taking it in the same apartment in which the type metal is melted.

Among one hundred type casters only three-fourths have not been attacked by painter's colic. This is to say, that one-fourth or twenty-five per cent. have suffered from the disease.

The types after being cast require to be smoothed and polished. Of all the persons engaged in type-founding, the polishers or rubbers are by far the most frequently attacked. Among one hundred and ten polishers at the type-foundry of M. Tarbé, M. Tanquerel found thirty-five who had been already attacked by

quently may serve as means of adopting precautions against its deleterious influence. It is probably too extensively used in the different arts. But difficulties in the way of obtaining proper substitutes are so considerable that it seems a hopeless task to attempt either to banish it from use, or to attempt much to limit its applications. Meanwhile it is impossible to doubt that wherever the use of this metal seems likely to be attended with danger, no one but a very rash person would attempt to urge or defend its employment. The most recent proposition that was made, namely, that of employing it in the refinement of sugar, serves merely to show that there is nothing too absurd to be proposed by some persons, and nothing so dangerous that it cannot obtain defenders.

ART. II.—1. *Practical Observations on Midwifery and the Diseases incident to the Puerperal State.* By ALFRED H. M'CLINTOCK, M.D., F.R.C.S.I., Ex-Assistant of the Dublin Lying-in Hospital; Vice-President of the Dublin Obstetric Society; and Lecturer on Midwifery and the Diseases of Women and Children in the School of Medicine, Park Street; and SAMUEL L. HARDY, M.D., F.R.C.S.I., Ex-Assistant of the Dublin Lying-in Hospital, and Vice-President of the Dublin Obstetric Society. Dublin, 1848. 8vo. Pp. 360.

2. *Clinical Midwifery, comprising the Histories of Five Hundred and Forty-Five Cases of Difficult, Preternatural, and Complicated Labour. With Commentaries.* By ROBERT LEE, M.D. F.R.S., Fellow of the Royal College of Physicians, London; Physician to the British Lying-in Hospital and St Marylebone Infirmary; Lecturer on Midwifery at St George's Hospital. Second Edition. London, 1848. 12mo. Pp. 228.

3. *A Short Sketch of the Life and Writings of the late JOSEPH CLARKE, M.D.,* Vice-President of the Royal Irish Academy, and formerly Master of the Dublin Lying-in Hospital, &c. &c., containing minute results of his Private practice, extending over a series of forty-five years; including three thousand eight hundred and seventy-eight births. By ROBERT COLLINS, M.D., President of the King and Queen's College of Physicians in Ireland, formerly Master of the Dublin Lying-in Hospital, &c. &c. London, 1849. 8vo. Pp. 83.

4. *Parturition and the Principles and Practice of Obstetrics.* By W. TYLER SMITH, M.D., London, Lecturer on Obstetrics in

English language there are at least five treatises on clinical midwifery of great value and utility to the practitioner. These are the two Reports of the Dublin Lying-in Hospital, the first by Dr Robert Collins, and the present one by Drs M'Clintock and Hardy; the cases in midwifery, published by Dr John Ramsbotham, first in 1832,* and subsequently in 1842;† the Clinical Midwifery by Dr Robert Lee, and the Cases in Midwifery by the late Dr Green Crosse of Norwich. In these works the reader will find useful information upon all the emergencies and difficulties most likely to take place in midwifery practice.

The Sketch of the Life and Writings of the late Joseph Clarke, M. D., contains an interesting and instructive account of a most industrious and skilful physician-accoucheur. The services of Dr Joseph Clarke in giving precision to many of the departments of obstetrical physiology and medicine, are well known to all interested in the progress of accurate knowledge. In the present memoir, a condensed view of these services is placed before the reader by the nephew of Dr Clarke, namely, Dr Robert Collins. It would be doing justice neither to the subject nor to the author of this memoir, if we attempted more than to mention its nature and object, which is done chiefly to give this performance its proper position in the literature of obstetrical medicine and surgery. At a more convenient season we hope to be able to give more ample attention to a subject of great importance. Meanwhile, when we say that we have read this memoir two several times, our readers will allow that it must be a work of no common interest. It must be generally allowed that Dr Collins has rendered a most useful service by the publication of the interesting facts contained in this sketch.

The other works in the list are more of a systematic character; two of them have been for a considerable time before the profession; and one has acquired a reputation both elevated and extensive.

The work by Dr W. Tyler Smith is remarkable for the originality of its views, its general philosophical character, and the great ingenuity with which the author explains the theory of conception, impregnation, and parturition. One pervading and predominant idea in the work is the doctrine of the Reflex Function of the Nervous System. The application of the theory of reflex action to the explanation of the physiology of impregnation and parturition is the duty which the author undertakes; and it cannot be denied that he is an advocate, both well-informed and ingenious. All the ideas formerly entertained, of the doctrines hitherto taught

* Edinburgh Medical and Surgical Journal, volume xxxix., p. 453. 1833.

† Ibid. volume lxvii., p. 305. 1847.

little to add to the theory of menstruation at the present time. I should mention to you that Dr Power does not seem to have been aware of the observation of Cruickshank or Kerkringius. His essay contains the most striking evidence of an original genius. It must, however, be said, that the ova of Dr Power were almost as hypothetical as those of Kerkringius. He knew of no ova except the vesicles of Kerkringius and De Graaf, and he drew a wrong analogy in comparing the formation and expulsion of the membranous secretion in dysmenorrhœa, to the extrusion of the unimpregnated egg of the pullet; and while he recognised the analogy between the uterus, as the nidus of the human ovum, and the nest of the bird, he appears to have failed to see any analogy between œstruation and menstruation. With these drawbacks, which are not very important, considering the state of physiology twenty-five years ago, the classical essay of Dr Power is quite on a par with the last word of science, and it affords a very remarkable and beautiful instance of the advance of theory before fact, as we shall immediately see. His mind saw the inevitable necessity for the existence of ova, and he wrote as though he had actually observed them."—Pp. 79, 80.

"The theory of menstruation was very much ridiculed when first propounded by Dr Power. Its author was treated as a visionary, and I have heard he acquired among accoucheurs the name of 'Menstruation-Power.' His little book was severely scourged by the critics of that time; but the event is another proof, that whenever ideas of sterling worth are put forth, the world, even if it do not at once appreciate their value, is sure to find them 'after many days.' Dr Power himself seems to have been little careful about the result; he never appears to have discussed the matter, or even resorted to experiments; but to have calmly thrown his ignot of gold into the stream of time, there to be refined, instead of casting it into the burning fiery furnace of controversy, for that more rapid assay which original minds sometimes covet. Very recently, I paid a visit to him, at his residence at Westminster, and I am proud to say he was much interested in my own researches in the physiology of Parturition. I found him amusing himself with painting and composition. He showed me some geological landscapes, if I may so term them, from his pencil, the still-life and figures of which were composed from the study of geological strata and fossil remains; ichthyosauri, plesiosauri, and other monsters basked, or pursued their prey, in scenes belonging to a primeval world. I mention this to show the original turn of the mind to which we owe the true theory of menstruation. It is little to the credit of our profession or of science that he has never received any reward whatever for his beautiful discovery. Indeed, until recently, it was not known whether he was alive or dead. He has neither courted nor received distinctions and honours of learned societies, British or foreign; born of a family in whom medicine may be almost said to be hereditary, and of which, until lately, three generations of physicians were living, he has still no other title or distinction save the M.D. of his professional diploma."—Pp. 80–83.

exclusive attention of the practitioner to this superficial form of dysmenorrhœa.

But the most serious disorders of menstruation are the hysteric and epileptic convulsions, which are sometimes excited by the ovarian irritation. The first attack of epilepsy frequently invades women at the coming on of a catamenial period; young girls are liable to convulsive attacks on the first appearance of the catamenia; and in confirmed epileptics the fits are always most violent and prolonged during the accession of the periods. *Ovarian irritation is, I am persuaded, the most important of all the causes of epilepsy, in the female.* Ovarian irritation is also a fruitful source of hysteric convulsion, and there is no other state of the economy in which the fits of hysteria and epilepsy run so much one into the other, and where the diagnosis requires to be made with equal care. In these cases it is not so much the uterine as the ovarian excitement which is the cause of the convulsion. It often happens that the fits of epilepsy occur before the commencement of the uterine secretion, when there is little uterine disturbance, and that they cease immediately on its appearance, the secretion from the uterus effectively depleting the neighbouring ovaria.

The influence of the ovaria upon the intellect and the emotions, are as remarkable in diseased as in healthy states. At each catamenial period the temper is disturbed in women of irritable constitution; in some women almost to madness. Indeed the ovaria appear to be an exciting cause of insanity in unmarried females, in the puerperal state, and at the catamenial climacteric. At the catamenial climacteric a revolution of the emotions commences, which requires especial study. General hyperæsthesia and hyperemotion exist at this epoch, of the most distressing character, both to the patient and her friends. Not the least singular feature of mental disturbance from ovarian irritation, is the sudden appearance and subsidence of the disorder. The most violent mania may appear almost without premonition, and disappear as abruptly. But I can only allude to these subjects, and to the influence of the ovaria in chlorosis, anæmia, hysteria, and various other affections peculiar to the sex.

In all such affections the treatment must be, not merely that which removes uterine disorder, but that which relieves undue ovarian excitement and irritation, both in the intervals between the periods and during the periods themselves. In the pathology as well as in the physiology of menstruation, the first place must be given to the ovaria. Their pathological synergies are quite as decided as those of a physiological kind."—Pp. 89-91.

In Lecture Eighth Dr Tyler Smith applies the doctrine of reflex action to the movements of the womb during parturition. The lecture is ingenious and cannot fail to be perused with interest. The following passage is all that we can make room for.

"The duration of human pregnancy is well known to be a multiple

spinal centre and the utero-spinal nerves a state of excitability almost of a tetanic kind. All the motor actions of labour are immoderately increased. The whole surface of the parturient canal becomes intensely excitor, and first the liquor amnii, then the head and trunk of the fœtus, and the manipulations of the accoucheur, produce the most rapid and powerful contractions, so that every uterine pain amounts to a local convulsion. Unless the nervous energy becomes exhausted by the uterine actions of the earlier stages of labour, the excitability increases with the progress of parturition, and reaches its acme at the time of delivery. The physical pain is in proportion to the motor action, being increased by the great pressure exerted on the uterine nerves, and by the sudden and forcible impulsion of the presentation through the passages; in such cases, the morbid uterine motor action, and the injury done to the parturient canal, may be said to partake, the one of the nature of cramp, and the other of violent contusion.

“ Though the nerves of the ovaria are the primary excitors of the neural energy exhibited in parturition, there are other organs, independent of both uterus and ovaria, which exalt the excitability of the spinal centre and the utero-spinal nerves. Thus a loaded state of the rectum, excitability of the bladder, and certain states of the stomach, act upon the uterus, and produce excessive action of the latter organ. The most important of these sources of excitation is the rectum, which, when loaded, will sometimes derange the whole function of labour, rendering the contractile pains quite uncontrollable. During labour the uterus attracts, as it were, to itself, the reflex effects of local irritation in any part of the body.”—Pp. 216–217.

Irritation indeed of the ovaries, all good observers will allow to be a much more frequent condition than inflammation; and we cannot refrain from repeating the opinion expressed in last volume, when speaking of the hypothesis brought forward by Dr Tilt, that the pain which is felt at menstrual and other periods, though acute, is indicative not of inflammation but of irritation. The existence of inflammation as a condition into which the ovaries may fall we do not deny. It is a possible, perhaps not an unfrequent occurrence. All that we contend for is, that it is neither philosophical nor consistent with observation to ascribe to inflammation, all those conditions in the female organs which are attended with pain even of considerable severity.

We observe that Dr Fleetwood Churchill signifies his approbation of the opinion of Dr Tyler Smith, that the chief seat of neuralgic dysmenorrhœa is in the ovaries; (p. 60).

Before quitting this subject, we may be permitted to say, that among the writings of physicians and accoucheurs, ample evidence may be found to show, that painful mensuration, though it might seem to suggest to the minds of many inflammatory action, either in the ovaries or in the Fallopian Tubes, nevertheless, was generally believed to indicate the presence of that species of disorder which is now called irritation, neuralgia, exalted innervation. Many

authorities might be adduced to prove the statement now made. But we shall go no farther than to Dr John Fothergill, an eminent physician in London during the middle of the eighteenth century, and one of the great supporters of that Association of physicians and surgeons, to whom we not long ago said, the profession was indebted for the collection entitled the Medical Observations and Inquiries.

"Nothing, perhaps," says this physician, "is more excruciating to the patients, nor, in common, more difficult to cure than painful menstruation. It impairs their health at present, and seems to render them less prolific in future. To the sufferers it is a most serious evil." "This excruciating pain seems to be spasmodic, and to proceed from the extreme irritability of the uterine system. The blood naturally determined hither, in order to its being discharged, by distending the very irritable vessels, occasions the spasm; this produces a constriction of the vessels; they become impervious, and the *nixus* to the discharge continuing, the pain becomes exquisite and general, till the patient, worn out with the struggle, is debilitated and sunk. The glands are then dismissed; some case succeeds; but the patient is often so reduced as not to recover her usual strength before she has another conflict to undergo."

"The *Fluor albus* is frequently the consequence of this struggle; and it would seem as if the *Uterus* itself was so far a sufferer as to be rendered, by degrees, less fit for fecundation. I think it has been observed by other physicians as well as myself, that few of those who have suffered much in the manner here described have borne children."

Now, upon the hypothesis here expressed, any opinion may be entertained that readers think proper. But it is quite clear that here is the doctrine of irritation as distinctly expressed as can well be done. Even the expression *irritability of the uterine system* may be taken without difficulty to imply the irritable state not only of the uterus, but of the ovaries and Fallopian tubes. It is also a well-known fact that dysmenorrhoea and leucorrhoea, in a great many cases are conjoined, and that the latter is not cured until the irritable state of the uterine system has been removed.

The author proceeds in succeeding lectures to apply the doctrine of the Reflex Function to explain the phenomena of Rupture of the Womb, and Laceration of the Perinæum (lecture sixteenth); Inversion of the uterus, After Pains (lecture seventeenth); and Puerperal Convulsions (lecture twentieth, twenty-first, and twenty-second.) This latter portion may be said to be the most elaborate of the work; and it certainly presents a very plausible

* On the Management Proper at the Cessation of the Menses. By John Fothergill, M.D., F.R.S., Medical Observations and Inquiries, Volume V., Art. XVII. 185. London, 1776,

view of the physiological and pathological relations of convulsions in the puerperal state.

It would not be difficult, did our limits permit, to make from this small and ably written volume many extracts of interest. But enough has been said to show the general character of the work, and to induce, we trust, not merely obstetrical, but medical readers in general to peruse it. Whoever peruses it impartially and patiently will, we think, arrive at the conclusion that the author is a zealous and able advocate for the Reflex Function, and that he evinces a very considerable degree of address and skill in its application to the phenomena, normal and abnormal, of impregnation and parturition.

In giving this character of the work of Dr Tyler Smith, we refrain from expressing any opinion as to the truth or the well-foundedness of what has been called the doctrine of the Reflex Function. The facts which have been referred to this function, and which are believed to prove its existence, have been long well-known. These facts are probably most easily remembered and most readily understood by referring them to this head. But it must not be forgotten, that it is a name which is employed merely in reference to the mental faculties; and that after all, it is an attempt to explain by a mechanical act, and by comparison with mechanical acts, phenomena which are not thereby made in any degree more distinct, and which are necessarily of a nature so obscure, that though their effects are seen, their actual operation entirely eludes sensible observation of every kind.

5. The Theory and Practice of Midwifery by Dr Fleetwood Churchill is a treatise composed with great skill and labour, and containing an immense amount of useful instruction. Giving very correct views of all the facts and principles which require to be known to the scientific accoucheur, Dr Churchill has further greatly enhanced the value of his work by the ample collection of statistical facts and tabular results with which he has provided the student.

It is well known, that in the Principles and Practice of Midwifery considerable difference of opinion as to certain measures to be followed in particular cases, has been long entertained by the ablest and most intelligent practitioners. It is very likely that this difference in opinion will continue so long as Midwifery is a science; for there are in certain divisions of the subject inherent and insuperable difficulties which it may not be easy to remove. One of the most probable means of diminishing, if not removing and overcoming these difficulties, is to make extensive collections of facts, and from these to endeavour to educe fair average results. Though the knowledge of these averages by no means

removes the difficulty and danger for particular cases and emergencies, it makes the nearest approach that is practicable to doing so. It applies the doctrine of numerical averages to determine the probable results. It is always, nevertheless, a point to be ascertained, under which category the favourable or the unfavourable, the particular case is eventually to place itself. To this department of the subject Dr Churchill has devoted much labour and attention, and he has been at great pains to show the reader the evidence, by which he is to be guided in practical proceedings.

In this way Dr Churchill has studied to determine not only the most important varieties and difficulties in the process of parturition, but the applicability of all the great operations, and the results which they may be expected to afford.

6. The sixth work on the list is, as our readers may perceive, the production of an American United States Professor in the far West.

Dr Miller has been desirous to discover, as are most authors, satisfactory reasons for publication. He allows that there are numerous works on Midwifery already before the profession; and though he admits that all have excellencies, yet he adds that all have faults. "While they have disseminated knowledge and enlightened the highways and byways of practice, and thus been instrumental in achieving much good, they have unsettled the minds of practitioners in regard to points of deepest interest, and made a wreck of obstetrical nomenclature, as far as the presentations and positions of the fœtus are concerned."

Two serious charges are here brought against all the writers that have hitherto attempted to instruct the rising generation of accoucheurs. Dr Miller accordingly undertakes, in the present performance, to clear away all doubts and perplexities that have been engendered, he maintains, as to matters of fact. He is to give a full, correct, and lucid description of the mechanism of labour; this being the leading object which in composing his book he had in view.

The fetal presentations and positions, Dr Miller maintains, have been thrown into confusion and disorder, the denominations and language employed are not understood. In most countries, he says, there is, and in all countries there ought to be, some standard agreed upon relative to this part of obstetrical medicine; but how, he asks, can such agreement exist in a country like ours which tamely consents to receive its science as well as its fashions from abroad? The United States need, he concludes, "at this time a native authority to educe order out of confusion, and set up a national standard, under which all our practitioners may arrange themselves and be cemented by a common bond of union."

Dr Miller accordingly avows, that to review the whole subject

"The first is the most dilatory of all the stages of labour,—it being the preparation for what is to follow, and preparation, in most things, usually requiring a longer time than execution. It may, therefore, be reckoned a prime duty of the accoucheur to wait patiently, as a general rule, until this stage is naturally accomplished. But is there no limit to the patience he must exercise? Is there no dilatoriness on his part which is reprehensible? Must the first stage of labor be always left to take its course, unless there be some uncommon and palpable necessity for interference?

"These are important questions, and their solution, more than anything else in obstetrics, settles the practical complexion of each individual to whom they are propounded. Hear the answer of Dr Denman;—'*Whether a short or a long time* be required for this purpose (the dilatation of the *os uteri*), it is the duty of the practitioner to abstain from interfering in this part of the process. It may sometimes be necessary to pretend to assist, with the intention of giving confidence to the patient, or composing her mind. But all artificial interposition contributes to retard the event so impatiently expected, by changing the nature of the irritation and the action thereon depending, or does mischief by inflaming the parts, and rendering them less disposed to dilate; in short, by occasioning either present disorder or future disease.'* So spoke the British oracle, in the latter part of the last century; and, with two or three exceptions, his successors to the present time, have echoed the authoritative response. The published lectures of one of them, who is yet living;† are full of kindred advice, eloquently and vehemently enforced,—exhorting the pupil to abstain from every species of interference, unless urged by dire necessity; and turn where he may, almost upon every page, the apothegm stares him in the face,—'*a meddlesome midwifery is bad.*' The deepest impression which his teaching is calculated to make, is, that nature is particularly careful not to be at fault in the matter of childbirth, and that it is a fearful thing to do aught but admire her proceedings.

"I have said that one's cast as a practitioner is determined by his views of professional duty, in the management of the first stage of labour; if he perform the part of an *expectant* in the first act of the drama, it is not likely that he will appear in any other character in the subsequent acts. Now, it is notorious that Dr Denman, for example, is hesitating and indecisive in the advice he gives, touching the conduct of the obstetrician in every branch of his duties; and after most carefully considering his views, one can hardly determine whether, upon the whole, he had best to do something or nothing.

"With the tenets of Dr Denman contrast those of the late Professor Hamilton, of Edinburgh, who states that he was but a very

* Introduction to Midwifery, chap. ix. sec. 6.

† Lectures on the Principles and Practice of Midwifery. By James Blundell, M. D., edited by Charles Severn, M. D. Philadelphia, 1842.

short space of time in practice, when he saw reason to believe that the management of the first stage had been much misunderstood by the profession. Observing that when the natural powers alone are trusted to, this stage is often greatly protracted, he inferred, from reasoning upon the subject, that injurious effects must be the consequence. 'He considered,' to use his own language, 'that upon the occurrence of every uterine contraction, there must be a certain influence on the action of the heart and arteries, and that if pain and sleeplessness were continued beyond a limited time, there must be an exhaustion of sensorial power. He concluded, therefore, that where the first stage of labor is not completed within a certain time, the strength of the patient must be proportionably lessened, the uterine action must be enfeebled, and the circulation of the blood must be disturbed.'[†] But, finding that the most respectable practitioners, both British and foreign, deprecated all interference with the first stage of labor, he felt diffident in promulgating his opinions, and did not do so until the year 1800, when he stated as the result of his observation and experience, for about fifteen years, that 'unless the first stage of labor (supposing that there are regular pains) be completed *within twelve or fourteen hours* from its real commencement, the following consequences may be dreaded.

"Firstly. That the powers of the uterus may be inadequate to expel the infant with safety to its life, or to the future health of the parent.

"Secondly. That after the birth of the infant, the uterus may contract irregularly, so as to occasion the retention of the placenta.

"Thirdly. That after the expulsion of the placenta, the contractions of the uterus may be too feeble to prevent fatal hemorrhage. And,

"Lastly. That, supposing the patient should escape all those untoward circumstances, febrile or inflammatory affections of a most dangerous nature may ensue, from the previous protraction of pain and the irregular distribution of the blood.'

"In view of these facts, Dr Hamilton adopted it as a rule of practice, that *the termination of the first stage of labor should be secured within twelve or fourteen hours from its actual commencement.*

"In prescribing limits to the first stage of labor, he is careful to guard against any mistake or abuse that might grow out of the restriction. There must be a continuance of regular pains for the period specified, 'for it sometimes happens that, after regular pains have commenced, the agitation of the patient, or the mismanagement of the attendants, occasions a suspension for some hours. If there be no injurious pressure upon the passages during that suspension, the patient's strength is recruited, and the duration of the first stage is to be reckoned from the recurrence of the pains.' And then again, spurious pains are to be discarded from the estimate; these may precede the true ones for hours, or days, producing no tightening of

* Practical Observations on various subjects relating to Midwifery. American edit. part i. p. 60.

the edges of the *os uteri*; and unless this is present, labour has not really commenced.

“ By the adoption of this rule, the author asserts confidently that ‘ *no patient under his charge, for the last thirty-five years, has been above twenty-four hours in labour, and, except in cases of disproportion, none so long.*’

“ Such a result as this,—so encouraging to those who are expected to alleviate the sufferings, and abridge the anguish of the sex, and who derive unfeigned pleasure from the fulfillment of their responsible mission,—challenges the candid consideration of every one, who is qualifying himself for the practice of obstetrics. It is entitled to the sober second thoughts of such as are already engaged in practice under a different creed; but, alas! with how little prospect of success can they be appealed to, since the principles, from which this result was obtained are condemned as heretical by nearly all authors, except Professor Burns. The latter even abridges the period prescribed by Dr Hamilton, within which the first stage of labour should be completed. ‘ If,’ he says, ‘ the pains be continuing without suspension, for an interval of some hours, and the labour be going on all the time, but slowly, it is a good general rule to effect the dilatation of the *os uteri* within *ten or twelve hours* at the farthest, from the commencement of regular labour.’* In relation to the necessity of this he speaks thus confidently. ‘ It is an undeniable proposition, that there is in every case a period beyond which it cannot be protracted without exhaustion; and it is no less certain, that if we wish to avoid this exhaustion, which may be followed by pernicious effects, we have only the choice of either suspending the action altogether for a time, or of endeavouring to render it more efficient, and of effecting the desired object within a safe period.’

“ The doctrine of Hamilton and Burns, on this subject, I have ever considered sound in the main, and their practice worthy of imitation. It is, therefore, incumbent on me to explain the one a little more fully, and defend the other. The doctrine assumes that the uterus is incapable of continuing its parturient action beyond a definite period, notwithstanding its frequent respites, without falling into a state of exhaustion that unfits it for the proper performance of its function. And does any one doubt the truth of this assumption? If so, let him refer to any treatise on practical midwifery, worthy of its title, in which there is not a distinct recognition of a *powerless state* of labour,—not that the uterus is so completely exhausted as to cease its action altogether, but it is so enfeebled as to be disqualified for efficient action, and the delay and danger, incident to such a state, call loudly for assistance. It is worthy of observation that before the uterus reaches this deplorable state, it is liable to become morbidly irritable and sensitive, so that it is disposed hastily to eject its contents, the instant the obstacle is removed, which provoked this morbid state. This irritable condition, as I may have occasion

* Principles of Midwifery, American edition, with Notes by Professor James, vol. i. p. 417.

to show, is scarcely less dangerous than the exhaustion of which it is the precursor.

"But although it may be conceded that the uterus is liable to exhaustion from long-continued exertions, it may be denied, as in fact it has been by Dr Churchill,* that the pains of the first stage have any such pernicious tendency. In his chapter on 'Tedious Labour,' Dr Churchill lays down the following propositions: '(1.) When the delay is excessive, the *relative* duration of the two stages is destroyed, so that they bear no steady proportion to each other; thus, for instance in a labour of sixty hours, the first stage may occupy fifty-nine, the second only one, or *vice versa*; (2.) That the effects of a prolonged labour upon the constitution of the patient, depend upon the stage in which the delay occurs; and (3.) That delay in the first stage involves very little if any danger, no matter how tedious it may be, but that delay in the second stage, beyond a comparatively short time, is always serious of import.

"These deductions, Dr Churchill thinks, are involved, though not distinctly enunciated, in the practical remarks of writers on midwifery, who distinguish the cause of delay in the first stage from those in the second, as being much less, dangerous: and in further corroboration of them, he publishes a tabular synopsis of one hundred and forty-three cases, to exhibit the relative duration of each stage, in labours of twenty-four hours and upward, in which the delay occurred in the first stage, and the results to mother and child. The table offers such cases as the following;—first stage $34\frac{1}{2}$ hours, second half hour,—first stage $41\frac{3}{4}$ hours, second quarter hour;—first stage $59\frac{1}{2}$ hours, second half hour,—first stage 176 hours, second 1 hour. Notwithstanding the tediousness of the labours, all the mothers recovered, and but ten of the children were lost, one of which was putrid. It is plain that these statistics, supposing them to be accurate, are fatal to the views of Professor Hamilton; for it would appear from them that the powers of the uterus are not enfeebled in the slightest degree, by the utmost prolongation of the first stage; on the contrary, the second stage is, notwithstanding, executed with remarkable facility, and without involving the least risk, present or prospective, to the mother; the only disadvantage being the loss of a larger proportion of the children, than in labours performed in better time.

I was, I confess, greatly astounded when my attention was first directed to these statistics; not because they militated against a favourite doctrine or theory (that I could have easily surrendered at the summons of truth), but because if they are to be relied on, I have yet to learn the alphabet of practical midwifery. Experience for more than a quarter of a century (a long time to be under delusion), has seemed to teach me that if the first stage be unusually protracted, the second is *liable* to be so too; or where it is not correspondingly delayed, it is *liable* to be executed so precipitately as to endanger both mother and child,—the uterus having acquired the

* *Theory and Practice of Midwifery*. Philadelphia, 1846.

morbid irritability to which I have alluded. In the latter case, irregular contraction is apt to follow the expulsion of the infant, producing difficulty in the third stage.

"In rehearsing my experience, in general terms, I have been careful to say 'liable,' because there is no rule without exceptions, and it does sometimes happen that tedious first stage is followed by safe and reasonable promptitude of the second. These exceptions are not, however, numerous enough to account for Dr Churchill's one hundred and forty-three cases, which he says expressly were not culled for the occasion. How, then, are they to be accounted for? I could never have answered the question, if, in turning over his pages, I had not stumbled on his definition of the first stage, which is as follows:—'*extending from the commencement of labour to the passage of the head through the os uteri*'" (p. 199). When the head has passed through the os uteri, it is, I opine, near its journey's end; for nothing is more common than for the anterior edge of the os uteri to be felt in advance of the head, a few moments before it emerges, although the pains may have been decidedly expulsive, that is, the second stage existed, for hours previously.

"The complete dilatation of the os uteri, spoken of as the end of the first stage, is never its obliteration, so that it cannot be felt and does not even require a degree of force to engage the head in it,—but such an opening of it as will allow the head to pass with no more resistance than the other soft parts, the vagina, perineum, and vulva, ordinarily offer,—for labour is a struggle throughout. Before the os uteri is dilated to this degree, the membranes protrude through it, and with *sufficient* dilatation of the orifice and the *formation* of the membranous pouch, the first stage of labour closes, and the second is ushered in by *expulsive* contraction, only slightly different at first from the *propelling* contractions of the former.

"According to my understanding of what belongs to the several stages of labour, then (which accords with the best writers); Dr Churchill's second stage is indebted for its remarkable brevity to his gratuitous bestowal of a goodly portion of it upon the first stage, and his table is, therefore, valueless and proves nothing. I shall only add that, in this same chapter, he concedes the point in dispute when he allows, that 'undoubtedly a prolonged first stage is a bad preparation for any accidental complication of the second.' Why is it 'a bad preparation,' but because the patient is fatigued and worn out with unprofitable suffering, and the energy of the uterus is impaired?"—Pp. 127–136.

7. In the fifty-fourth, and afterwards more fully in the fifty-seventh volumes of this Journal, the nature and merits of the Principles and Practice of Obstetric Medicine and Surgery were placed before the profession. The opinions at that time expressed have been fully realized. The work has been most favourably received both in this country and in the United States. A second edition was published in 1816, as already noticed, in volume sixty-seventh;

and it is stated, that since the first appearance of the work, in its completed form, in 1841, the number of four thousand five hundred copies have passed through the hands of the publisher.

Though the extensive demand for a work on any department of medical knowledge, cannot in all instances be received as a proof and test of the merits of the work; yet, as in this instance, the merits of the work are undeniable, it is gratifying to know that they are so generally and unambiguously admitted.

In the present edition, Dr Ramsbotham has, with his great and intimate knowledge of the subject, exerted his usual skill and judgment in adapting the work to the advancing progress of midwifery as a science and as a liberal art.

In the second edition, published in 1846, Dr Ramsbotham introduced much practical information upon the *diseases of the puerperal and pregnant states*, and various points were illustrated by statistical tables from the practice of the Royal Maternity Charity. In the present edition, the author has added to this division of the work, and rendered it still more serviceable to the practitioner. This division extends from the 515th page to the end of the 667th.

In the present edition, Dr Ramsbotham introduces a judicious and temperate examination of the question of the induction of anæsthesia in labour, and the circumstances which may seem to indicate or to contraindicate the administration of chloroform. Dr Ramsbotham, after careful and dispassionate examination of all the circumstances, and considering the probable effects, arrives at the conclusion that chloroform is an agent which ought not to be administered in ordinary circumstances of labour to parturient females. His final conclusions are stated in the following manner.

“No doubt it is a great bait to offer, and a great boon to promise to a woman who is looking forward with anxiety to her approaching confinement,—that she is to be lulled into a sweet, quiet, placid sleep,—to be perfectly unconscious of all the distress and suffering attendant on her labour;—and to be recalled to the world only by the grateful cries of her infant. But if the case were put fairly and honestly before them;—if they were informed that they might probably be made ‘*dead-drunk*,’ but must certainly be reduced to that condition which the law designates as ‘*drunk and incapable*,’ how many, it may be asked, of our high-born dames,—how many a woman possessing common feeling,—how many indeed removed above the very lowest orders of society, would be found to avail themselves of the immunity from suffering which anæsthetics hold out, at such a price, and at such a sacrifice of moral obligations? I cannot help thinking, and I may add also, hoping, for the honour of female nature, that those who have acceded to the proposal have been betrayed into acquiescence by the belief that they were to be merely steeped in sleep, and not drowned in intoxication. And yet nothing seems

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more clear than that they must be cast into the intense stupefaction of drunkenness, before they can be rendered oblivious to the mental anxieties or corporeal sufferings incidental to their situation.

"I must repeat emphatically, that this state is not sleep, but drunkenness. The only kind of sleep, if sleep it can be called, in the least degree analogous to it, is that death-like insensibility into which a person is hurled when stupefied by spirituous liquors:—a state but little removed from apoplexy; and which indeed in very many instances has eventuated in a seizure of that dreadful malady. Even from the stupor induced by opium, a patient can be roused by loud and sudden noise,—as the firing of a pistol,—or by violent usage; but no power on earth could recal to his senses and sensibilities one overwhelmed by the deadening influence of the vapour of chloroform, so long as it retains within its grasp the unconscious object of its irresistible might."—Pp. 179, 180.

The whole discourse, however, should be perused, and will, we cannot doubt, be perused carefully.

On several other subjects, as the employment of galvanism in lingering labours, the operation of turning in cases of head presentations where the pelvis is small, and on the removal of the placenta before the infant, where that body is implanted over the *os uteri*, Dr Ramsbotham has added various instructive observations.

In conclusion, this work sustains with great ability the character originally given. It may be justly recommended as a Treatise on Obstetric Medicine and Surgery of a very superior order.

ART. III.—1. *Translation of the Pharmacopœia of the Royal College of Physicians of London*, 1851. *With Notes and Illustrations.* By RICHARD PHILLIPS, F. R. S., L. E., F. G. S., Late President of the Chemical Society, Curator of the Museum of Practical Geology, &c. London, 1851. 8vo., Pp. 567.

2. *The Pocket Formulary and Synopsis of the British and Foreign Pharmacopœias. Comprising Standard and Approved Formulæ for the Preparations and Compounds employed in Medical Practice.* By HENRY BEASLEY. Fifth Edition. Corrected, Enlarged, and Adapted to the Last Editions of the Pharmacopœias. London, 1851. 12mo., Pp. 546.

IN the article in last volume on the London Pharmacopœia and the Commentaries on that publication, no information had been communicated to us, whether the late Mr Phillips had been engaged in a translation of the Pharmacopœia or not. It was consequently not in our power to speak with any confidence at that time as to

Mr Smith has been both industrious and successful, and many of the commentaries given by him show how much the work owes to his knowledge. Of this, readers who peruse the volume with care, will observe frequent proofs.

On the important subject of Tests, a department in which the edition of 1836 was very complete, every thing has been done to render them both trustworthy, and at the same time of easy application. This subject acquires additional importance in the present work, from the circumstance, that as the College do not insist, that the medicines and chemical preparations ordered should be prepared in the exact mode prescribed in the *Pharmacopœia*, providing they are of the required purity and strength, the only mode of determining these qualities is by the use of accurate tests.

In the article in last volume, we stated in general terms, that several formulæ or directions for the preparation of chemical articles had been removed from the Formulary, either because they were supposed to be there out of place and unnecessary, or because the articles had been transferred to the *Materia Medica* List, and consequently were understood to be no longer prepared by the apothecary. On the propriety of this proceeding different opinions will be entertained by different individuals. It may be doubted, so far as the actual preparation of chemical medicines is concerned, whether any of them can be advantageously prepared, as in former times, by persons engaged as medical practitioners. But whether this be admitted or not, it is impossible to deny, that physicians and apothecaries ought to know how these articles are prepared, to understand the chemical principles by which their preparation is regulated, and should possess a sufficient amount of chemical knowledge to be able to say when they are pure or impure, well prepared or imperfectly prepared. A competent knowledge of chemical pharmacy is, in short, not only a necessary branch of medical education; but the possession of such knowledge invariably makes its possessor a more useful member of the profession than one without it.

With the importance and the truth of these facts the late Mr Phillips appears to have been deeply and justly impressed. The removal of all the formulæ of 1836 from the *Pharmacopœia*, would, we think, have taken away the material elements of a considerable proportion of useful, chemical, and pharmacological knowledge. To obviate this result, the method was adopted by Mr Phillips and adhered to by Mr Smith, of inserting in the present translation, not only all the Formulæ of the present new *Pharmacopœia* of 1851, but all the most important Formulæ of the edition of 1836. The reader will observe in various parts of the volume, *Formulæ* printed in Italian characters. These *For-*

OMISSIONS

Materia Medica.

Simaruba.
 Sodæ Acetas.
 — Carbonas impura.
 Spigelia.
 Stannum.
 Succinum.
 Terebinthino Canadensis.
 Testæ.
 Toxicodendron.
 Tussilago.

Preparations.

Aconitina.
 Morphia.
 Carbo Animalis purificatus.
 Testæ Præparatæ.
 Aqua Fœniculi.
 Decoctum Malvæ compositum.
 — Veratri.
 Emplastrum Ceræ.
 Extractum Digitalis.
 Infusum Perciræ.
 — Scoparii.
 — Simarubæ.
 Argenti Cyanidum.
 Liquor Calcii Chloridi.
 Ferri Iodidum.
 Hydrargyri Oxydum.
 — Binoxidum.
 — Biniodidum.
 — Bicyndidum.
 — Sulphuretum cum Sul-
 phure.
 Plumbi Chloridum.
 — Oxydum hydratum.
 Liquor Potasse effervescens.
 Potassæ Bisulphas.
 Potassii Bromidum.
 Liquor Sodæ effervescens.
 Mistura Assafœtidæ.
 — Cascarillæ composita.

INSERTIONS.

Preparations.

Liquor Arsenici Chloridi.
 Syrupus Ferri Iodidi.
 Ferri Carbonas cum Saccharo.
 — Ammonio-citras.
 Vinum Ferri.
 Liquor Sodæ.
 Zinci Chloridum.
 Pilula Aloes cum Sapone.
 Sulphuris Iodidum.
 Syrupus Cocci.
 — Violæ.
 Tinctura Aconiti.
 — Belladonnæ.
 — Cinchonæ Pallidæ.
 — Ergotæ Ætherea.
 — Limonum.
 — Lobeliæ.
 — — — — Ætherea.
 — Quinæ composita.
 Ammoniacum præparatum.
 Assafœtida præparata.
 Cassia —.
 Galbanum præparatum.
 Pix Burgundica præparata.
 Prunum præparatum.
 Sagapenum —.
 Styrax præparata.
 Tamarindus præparatus.
 Thus præparatum.
 Unguentum Belladonnæ.
 — Conii.
 — Hydrargyri Nitratis mi-
 tius.
 — Opii.
 — Potassii Iodidi.
 — Sulphuris Iodidi.
 Acidum Hydrosulphuricum, re-
 cens præparatum.
 Ammoniæ Oxalas, *Crystalli*.
 Aurum.
 Cuprum.
 Ichthyolla.

OMISSIONS.

Preparations.

- Moschi.
- Oleum Origani.
- Succini.
- Terebinthinæ purificatum.
- Pilulæ Hydrargyri Iodidi.
- Sagapeni compositæ.
- Alcohol.
- Spiritus Ammonia.
- Lavandulæ.
- Tincturæ Camphoræ
- Cardamomi.
- Guaiaci.
- Unguentum Hydrargyri mitii.
- Hyrargyri Biniodidi.
- Veratri.

INSERTIONS.

Preparations.

- Liquor Chlorinii, recens præparatus.
- Indigo Sulphatis.
- Platini Bichloridum.
- Poissii et Hydrargyri Iodocyanidum.
- Stanni Protochloridum.

We have now mentioned as fully as the limits of this notice will permit, the principal characteristics of this translation of the Pharmacopœia of the London College. The commentaries, which are to the student the most instructive part of the work, are given with great clearness and accuracy. With the explanatory diagrams it cannot be difficult for any one of moderate attention, to understand the correct views of chemical action in pharmaceutical products. We therefore recommend the work in strong terms to all those who are engaged in the study of chemical pharmacy. To those who already possess the Pharmacopœia, it must be useful as a commentary and explanatory guide. To those who do not possess the Pharmacopœia, it may serve as a substitute for that volume.

. Of the Pocket Formulory by Mr Beasley, we have merely to say that it is one of those portable works of which we had occasion to speak in last volume. It is arranged in alphabetical order, by which means facility of reference and consultation is favoured. The different articles contain many curious and serviceable formulæ.

At the end is given a list of antidotes and articles employed as counter-poisons, a short account of weights and measures, and a table showing the proportion of active ingredients, as antimony, mercury, opium, and hydrocyanic acid, in certain compound medicines.

As a portable dictionary of *Materia Medica* and Pharmacy, the work is calculated to be useful to the practitioner who has already studied the subject, and perhaps, as the author expects, as a counter companion to the dispensing chemist.

After the author has considered at due length the pathological history and the therapeutic principles of these general and common subjects, he enters on the history of the proper and specific Surgical diseases.

First come Diseases and Injuries of Bones. The ordinary Diseases are explained in the fifteenth and sixteenth lectures; the Malignant Diseases in the seventeenth. The six following lectures, from the eighteenth to the twenty-third inclusive, are devoted to the extensive and complicated subject of Fractures.

In the twenty-fourth, twenty-fifth, and twenty-sixth Lectures, is delivered the pathological history of Diseases of the Joints, with the treatment appropriate to each. In the next four Lectures, from the twenty-seventh to the thirtieth inclusive, Mr Cooper explains the peculiar characters of the various forms of Dislocation in the superior and lower extremities.

In the seven following Lectures, from the thirty-first to the thirty-seventh inclusive, the important subject of the Surgery of the Regions is considered. In this division Mr Cooper explains fully the diseases and injuries incident to particular regions of the human body, and describes the treatment, whether medical, surgical, or operative, which each affection and accident demands.

Hernia and its different varieties are fully delivered in four lectures, from the thirty-eighth to the forty-first inclusive.

Next follow Diseases of the Rectum, in lectures forty-second and forty-third; Diseases of the Genito-Urinary system in lectures forty-fourth and forty-fifth; and the extensive subject of Stone in the Bladder in the next four lectures to the end of lecture forty-ninth.

Four lectures are devoted to Diseases of the Genital Organs; three to Diseases of the Breast; seven to Lesions of the Vascular System; one to Neuralgia; one to Tumours; six to Amputation; one to Distortions of the Limbs; and three to the Venereal Disease.

It is impossible in this place to enter into any detailed account which could do justice to the contents of this volume. We feel it, however, to be our duty to say, that Mr Cooper has provided for the surgical reader a volume full of instruction. The author expresses himself in very diffident terms as to the success of his work; and though he thinks it has merits as a practical performance, he seems willing, if the profession do not confirm this opinion, that his work should go into the oblivion which it would in that case deserve. This apprehension, we trust, will not be realized. If the book is neglected, it must be owing to causes unconnected with its actual merits as a didactic work, which, in our opinion, are very great.

and fingers, there is the same opaque yellowish discoloration. The appearance is much as if the cuticle were thickened, and the disease confined to it; but, on a complete investigation, it is evident that here, as on the face, it is healthy, and that the morbid change is seated in the cutis, which is rather thickened, altered in colour, and has increased sensibility. The disease remained stationary until death, at the end of four years from the beginning of the jaundice. Towards the end, the colour of the general surface deepened to a mahogany-brown. No affection of the skin, similar to that described on the face and hands, appeared elsewhere.

A model of this is also preserved in the museum.

3. On the 18th of August 1848, a patient, labouring under diabetes, was admitted into the hospital. The person, John Sheriff, was twenty-seven years of age, of middle stature, by occupation a tailor, residing near Kingsbridge, in Devonshire. About six months before, he began to pass an unusual quantity of water, feeling at the same time weak and feverish, with a dry harsh skin. On admission, he presented the ordinary symptoms of diabetes; he voided four pints and a-half of urine daily, *sp. grav.* 1050. The treatment pursued was various, but without any obvious improvement. On the 25th January of the following year (1849), the quantity of urine was seven pints and a-half, *sp. grav.* 1042. At this time an eruption somewhat suddenly appeared on the arms, at first apparently of a lichenous character. In the course of ten days it had extended over the arms, legs, and trunk, both anteriorly and posteriorly, also over the face and into the hair; it consisted of scattered tubercles of various sizes, some being as large as a small pea, together with shining, colourless papules. They were most numerous on the outside and back of the fore-arm, and especially about the elbows and knees, where they were confluent. Along the inner side of the arms and thighs they were more sparingly present, and entirely absent from the flexures of the larger joints. Besides the compound character produced by the confluence of two or three tubercles, many of the single ones had also a compound character, or appeared to have such, as shown by the prominent whitish nodules upon them. Some looked as if they were beginning to suppurate, and many were not unlike the ordinary molluscum, but when incised with a lancet, they were found to consist of firm tissue, which, on pressure, gave out no fluid save blood. They were of a yellowish colour, mottled with a deepish rose-tint, and with small capillary veins here and there ramifying over them. They were accompanied with a moderate degree of irritation, hence the apices of many were rubbed and inflamed. The nature of the eruption gave rise at the time to much discussion. On its first appearance, some suspected it to have a secondary venereal affection; but there was nothing in the case, nor indeed in the character of the eruption, when carefully examined, to support this view. The only cutaneous affection with which we could associate it was that of a young woman, whose case we have given above, where the tubercles had occurred in the face only. The eruption continued almost stationary from the end of January to the beginning of March, when many of the tubercles began to subside, having no obvious change in the texture of the skin. At the end of March the patient left the hospital, and the further course of the case was not ascertained. The appearances presented by the eruption in this case are well shown in model 2733th.

4. Eliza Parachute, *æt.* 33, of middle stature, moderately well nourished; mother of six children; catamenia regular. Her present illness began in 1848; she attributes it to fright, and to a blow received in the left groin whilst attempting to separate two men who were fighting. Two

days after this she became jaundiced, and had from time to time severe paroxysmal pains about the hypochondria, lasting for a day or two; the liver being also enlarged and tender. Four months after the commencement of the jaundice, (August 4th, 1848,) she was admitted into the Hospital under the care of Dr Hughes. She remained in until the 26th of September, and left much in the same state she was in when admitted. There was at this time nothing complained of beyond the itching and irritation of the skin common in jaundice. The present affection began after the jaundice had continued fourteen months, when she again came under the care of Dr Hughes. It first appeared in the hands, spreading across the flexures of the joints of the fingers and palms. Soon afterwards a yellowish patch of discoloration began near the inner canthus of the eyelid, and then a precisely symmetrical one at the same part on the opposite eyelid. These patches are very slightly raised, and not obviously indurated; they have extended very slowly. In the early part of the year 1850, two models 2733rd 2733th were made of the case. At this time the patches on the face existed as above described. Along the ridges bounding the flexures in the palm and about the joints of the fingers, there were yellowish, opaque, irregular, and somewhat raised lines. About the thumb, first joints of the fingers, and inner and interior parts of the wrists, there is a gradual transition to a tubercular prominence of the affected parts, and some distinct tubercles exist on the elbow and knee. The diseased parts are tender, so as to give her pain in using a knife to cut bread. The whole surface of the body is of a dull lemon tint. Various means were employed without avail, the disease showing a tendency to progress slowly. Through the kindness of Mr Startin, under whose care the patient now is, we have been able to observe it up to the present time. The jaundice still remains occasionally deepened by the exacerbation of the hepatic symptoms. The skin is of a dull lemon hue. During the last seven months the affection has become more tubercular, especially about the back of the joints of the fingers of the right hand. The patch of confluent tubercles on the elbow (Plate II., fig. 2) has much increased since the model was taken. Both elbows are similarly affected. There are also tubercles on the right knee, on the superior surface of the great toe, and on both ears. On the hands the gradations from the plane to the tubercular variety are well marked, and the essential relations of the two forms demonstrable. This case has been of the greatest value in enabling us to connect together the cases which had previously occurred. The tubercles about the ears, elbows, joints of the fingers, &c., are of the same character they were in Sheriff's case. They are firm, rather irregular on the surface; have much the appearance, at first sight, of small compound follicles, but on closer inspection are proved to depend upon a change in the cutis. On the surface small venous capillaries may be here and there seen, producing a mottled appearance. In the hands we pass insensibly from the tubercles on the back of the joints to the state described in Mrs. B's case, namely, the slightly raised, opaque, yellowish lines about the flexures of the palms and fingers. The further identity of the disease in the two cases is shown by the presence of similar patches about the eyelids in both.

Of this eruption a plate is given.

5. Mrs. J., æt. 43, of spare frame, and below the middle stature, married; mother of two children, and in good health, until about eight years ago, when her catamenia ceased, probably from fright. After their cessation she was never well, had pains about the right side and through the shoulders; and for several years past, indeed nearly ever since the commencement of her ailment, has been jaundiced. She was constitu-

anxious, and he was spasmodically affected at the approach of any one, at such a time he appeared choking. Heart's action normal; pulse soft, irregular, and 60; tongue slightly coated, but moist; skin natural; his breath was of an acid odour, and every two or three minutes he was seized with violent sighing. At a quarter before 10 A. M. he had a turpentine injection administered. At 1 P. M. Dr Babington saw him, and ordered Ferri Sesq. Ox. $\mathfrak{z}\mathfrak{j}$.; Syr. Aurant. q. s. ut fiat bolus, to be taken every quarter of an hour. The spine to be rubbed with Tr. Lyttæ, and belladonna plaster to be afterwards applied to the stimulated part.

At half past 2 he sank into a sleep, which lasted three quarters of an hour.

At 5 P. M. he was much prostrated, and the spasms increased. Pulse 54, irregular, and intermittent. He experienced the feeling of a ball rising in his throat. His urine, which he passed three or four times, was rather high coloured.

At 7 P. M. he was more restless. Mouth parched; skin hot and dry; pulse 64. The boluses were divided into three parts, and after each he took about two ounces of milk, the only nutriment taken since his admission. He has great difficulty in swallowing, and is very much convulsed.

At 10 P. M. his pulse was 54, but rather fuller; he could hear no light in his room, nor any one to approach him except his wife. His voice was natural, and he had no rigidity of muscle. He was ordered Morph. Hydrochl. gr. iij. every half hour till some effect should be produced. Unguent. Lyttæ was rubbed in over his chest about the region of the diaphragm, and afterwards belladonna plaster. He soon became more quiet, and slept from eleven till three. He passed a quiet night upon the whole, crying out at intervals. At 5 A. M. Sept. 27, he felt rather sick. He asked for a cup of milk, but swallowed it with difficulty. The bowels had not been open since his admission. The morphia was repeated.

At 8 P. M. he asked for some water, swallowed about two ounces, but immediately jumped up in bed and appeared choking. The morphia was repeated.

At 10 P. M. he was very violent and delirious. Pulse 120. He wished for water, but could not swallow it. Dr Babington ordered the Morph. Hydrochl. to be increased to four grains every half hour. He was confined to his bed with sheets.

12 P. M.—He still cries for drink, which, when taken, makes him sick. Pulse 144. The voice is thick. Breathing more laborious, and feeling of depression much increased. He displays no greater horror of fluids than of solids. Dr Babington ordered the dose of Morph. Hydrochl. to be increased to eight grains every half hour, and a tobacco enema to be given immediately.

About two o'clock the countenance was livid, and the angles of the mouth constantly raised and lowered. He made a noise like the howl of a dog. He breathed only eight times in a minute. Pulse small, 180. Occipito-frontalis contracted. He appeared unconscious, but now and then a calm smile played upon his face, as though his ideas were of a pleasing nature. The pupils were much contracted, and inobedient to the stimulus of light. Conjunctiva injected. He twitched the bed-clothes, and pulled his neck about. From this time he gradually became worse till twenty minutes past three, when he died. Immediately after death his pupils were much dilated, and the corpse had a very cadaverous odour. A vein was opened, but did not bleed. The next morning, however, he was deluged in blood. He took altogether forty-eight grains of hydrochlorate of morphia.

Hydrophobia treated with Lead.

Samuel Soring, a Londoner, aged 35, was admitted into billet ward, under Dr Addison, on the 1st October 1837. He had formerly been addicted to drinking, but not of late years. For the last three weeks he had been in a desponding mood, so much so, that he occasionally kept his bed for a day or two, though he continued his employment, which was fish selling. About three months since he had been bitten by a spaniel. The wound was trifling, did not bleed, and caused only slight pain. On the day before his admission he was very ill, and spent a restless night. His symptoms on admission were;—An anxious and dejected countenance; breathing difficult and convulsive, and this more violent after any movement, draught of air, or sight of water; pulse 80, with little power; skin natural; tongue parched; great thirst; slight pain in the head; pupils contracted; bowels not opened since yesterday morning; a sense of suffocation and tightness across the throat. At half-past 10 A. M. Dr Addison saw him, and ordered him to be cupped over the epigastrium. The cupping caused so much excitement, that when eight ounces had been drawn, it was thought advisable to desist. Ordered also, *Hæm. Sennæ c. Ext. Col. c. 3j.* as an injection, and two drops of croton-oil to be taken every half hour till it operated. The enema was soon returned with a small quantity of feculent matter. He swallowed his medicine with great difficulty, wishing to have his eyes closed while taking it. A composition of *Ext. Belladon. 3ij. and Ung. Hyd. 3j.* was rubbed on his throat. At 3 P. M. Dr Addison again saw him. The croton-oil had acted powerfully, the stools watery and green; skin cold and wet; pupils dilated; urine scanty, and priapism present; purging still violent. Ordered *Vin. Opii 3j. statim*, and *Liq. Plumb. Diacet. mxl.* every hour. The purging ceased shortly after taking the opium. At 10 P. M. Dr Addison again saw him. After the first three doses of the lead he seemed more tranquil. He now complained of great thirst, and drank eagerly two half pints of porter. He still went on regularly with his medicine, which he was persuaded to take with great difficulty. He was constantly troubled with violent emotions, which became worse after each dose of the medicine.

At half past 2 A. M. of the following day he was much worse. He appeared greatly distracted, imagining himself surrounded by hideous objects. It was necessary to confine him to his bed, as he would not stop there without compulsion. After this he became unconscious of everything around him. He roared in a most awful manner. The muscles of his face were horribly distorted. A large quantity of frothy saliva poured from his mouth, his lips became livid, and he appeared writhing in an excess of agony. In this state he remained till half-past four, when he became suddenly quiet, and appeared quite exhausted. Five minutes afterwards he was dead.

Hydrophobia treated with Prussic Acid.

Emmanuel Soult, aged 7, was admitted into King's College Hospital, under Dr Todd, on the 22d of November, 1841. Two months before admission he had been bitten by a mad dog under the right eyelid. The wound bled profusely, but rapidly cicatrized, and no constitutional disturbance was manifested till the day preceding his admission. He then exhibited a strangeness of manner, and slight symptoms of pyrexia only, until, a few hours after his illness had commenced, he suddenly threw himself into his mother's arms, and screamed loudly. His eyes were bright and prominent, he frothed at the mouth, and his inspiration was jerked and audible. He tossed his arms about, and tried to vomit: refused to eat, drink, or lie down, but did not object to the presence of water. At night, the symptoms were the same, but exaggerated: and

on the following day, after his admission into the hospital, spasm odic twitching of the muscles of the face, neck, arms, and larynx was observed. He vomited saliva and viscid mucus. Over the chest was heard a loud muco-sonorous râle. The pulse was small, and 140. The skin was dry, but not hot. The tongue was maintained protruding from the mouth; there was also some intolerance of light. Pressure on the cicatrix of the wound did not occasion pain, nor was it red or swollen. The lower lip, at its right angle, was very tumefied, which his mother referred to his having bitten it. He evinced great sensibility to draughts of air, requesting those who spoke to him not to blow upon him.

At a quarter to 4 P. M., by Dr Todd's order, five minims of hydrocyanic acid (Ph. L.) were placed on his tongue. At four o'clock, the spasms being frequent, and the retching violent, the acid was repeated in the same quantity, and was continued every quarter of an hour till five o'clock. After the second dose he seemed better: he was more quiet; retching less frequent; and he passed water in bed copiously, and was sensible of its escape. This state lasted till four to five o'clock, when he again, on the entrance of several persons into his room, became excited. The spasms returned with increased violence, there being at one time slight opisthotonos. The retching was more constant, and he could no longer lie down. At five o'clock, Dr Guy, in Dr Todd's absence, directed 20 minims of the acid to be given; and five minutes after, 10 minims more, but without any obvious effect.

At half-past five, Dr Todd ordered another 20 minims of prussic acid. This producing little or no good, at Dr Guy's suggestion Dr Todd ordered a frigorific mixture to be applied to the spine and throat. This soon had the effect of alleviating all his symptoms. He became more quiet; the retching was less frequent; he did not foam so much at the mouth. The larynx and pharynx were free from spasm; but his feet getting cold, and his pulse falling, the ice-bags were removed, and hot bottles applied to his legs. He swallowed some ice, too, with greediness, after which his power of deglutition improved so much that he was enabled to take some wine and Liquor Opii Sedativus. At seven o'clock, as he was again becoming excited, the ice was re-applied, but again removed at eight, since the heart's action was then much depressed. From this time till eleven, he was remarkably free from spasm, and his deglutition good. His tendency to delirium, however, increased and at eleven o'clock Dr Guy tried the cold douche. Almost immediately afterwards, the pupils, which had been previously dilated, became extremely contracted; and in a few minutes the boy was dead.

The body was examined fifteen hours after death, the autopsy revealing congestion of the brain, spinal cord, and their membranes. The cerebral substance was somewhat softer than usual. The bronchial tubes and lungs were also congested. Stomach empty and contracted; pharynx injected; its follicles being rather large.

II.—MEDICAL SURGERY.

Observations on the Treatment of Anasarca or General Dropsy, by Puncturing the Legs. By JOHN HILTON, F. R. S. (Guy's Hospital Reports, Second Series, Volume Seventh, Part Second, p. 358.) 1851.

It is well known to physicians and surgeons, that the practice of puncturing the lower extremities in anasarca, though often attended with temporary relief, carries with it in general so much danger that it is almost uniformly deferred until there is no hope of relief by any other method, and when it is prescribed to be performed it has proved too often merely another mode of the fatal termination of the disease.

sure with the hand. Extensive subcutaneous incisions of the cellular tissue, by any lateral movement of the instrument, are to be avoided. These are not required for the purpose, and they may be hurtful by injuring a tissue of feeble vital powers, and already weakened by distension with fluid.

Mr Hilton prefers the outer to the inner surface of the limb, because the former is less traversed by lymphatics and veins than the latter, and because, when the patient is in bed, the apertures occupy a lower level than would be obtained on the inner surface of the limb.

Immediately after the punctures have been made in one or both limbs, each limb should be enveloped in a separate blanket, for the purpose of maintaining a proper temperature, and to prevent chafing of the skin by the one limb being rubbed on the other.

If any appearance of irritation and maceration of the skin by the fluid take place, it is good to make the fluid preserve an exact course away from the limb, by fixing upon the limb worsted threads, which, acting by capillary force, should be arranged in such manner that, by entering gutta percha or glass tubes, they may direct the fluid into these, from which it may be conveyed into receptacles outside the bed.

Mr Hilton enters into several other details regarding the after-management. But these may be easily understood by those who are in any degree accustomed to treat cases of anasarca in this manner.

The method proposed, and already tried by Mr Hilton, seems well calculated to obtain the advantages, and avoid the disadvantages of puncture, when it is deemed requisite to have recourse to this rather ambiguous remedy.

The great object, after the operation, should manifestly be to keep the patient as dry and as warm as possible. From some experience in cases of this kind we may be allowed to say, that this object will be most easily and certainly attained by placing below the patient a sheet, in two or four folds, and below this a large piece of oiled silk. This arrangement will enable the attendants to change the linen, if wetted, without causing general disturbance of the position, often a troublesome matter to dropsical sufferers. The centre of the bed also should be well elevated.

III. SURGICAL PATHOLOGY.

Case of Popliteal Aneurism cured by Pressure upon the Femoral Artery. By G. M. HUMPHREY, Esq., Surgeon to Addenbrooke's Hospital, Cambridge.. (From the Provincial Medical and Surgical Journal for October 1851.)

William Mitchell, aged 24, a seaman, from Wisbeach, was admitted into Addenbrooke's Hospital, on August 1st 1849, in ill health, with anxious pained expression, sallow complexion, pulse 104.

The ham and lower-third of the inside of the right thigh was occupied by a firm swelling, giving a circular measurement of eighteen inches and a-half; the left limb, in a corresponding situation, giving only thirteen inches and a-half. The swelling was most prominent a little above the internal condyle, and at the middle of the ham, uniformly firm, but communicating an indistinct sense of fluctuation. Its outline could not be clearly ascertained in consequence of an oedematous condition of the subcutaneous tissues. The integuments were natural, with the exception of a faint blush in places, and a few white starry spots, like cicatrices, which he said had appeared within the last few days. There was effusion into the knee-joint, raising the patella and still further distorting the limb. The part was exceedingly tender, and he suffered great pain in the swelling, and agonizing pains shooting down the limb; the skin of the leg and foot feeling, to use his expression, as if it were being burnt in a fire. He was restless, incessantly turning about to find a

October 4th.—Has for sometime moved about the ward on crutches swinging the right leg. Moves the knee boldly though not quite freely. The size of the swelling diminishes very slowly, the circular measurement being still 17 inches. It is soft and indistinctly fluctuating. Sensation exists over a greater space than it did, but he cannot move the foot and toes, which remain swollen. The femoral artery may be traced to the lower-third of the thigh. There is no pulsation perceptible in the tumour or in the tibial arteries. The œdema of the foot might prevent its being felt in the latter. Returned to his home.

In the course of the following winter he was again in the hospital for several weeks, on account of the paralytic condition of the foot, which continued, although the aneurismal tumour was smaller and soft, and pulsation had entirely ceased. Electricity was used, with some benefit, and he again went home. Some months afterwards he was attacked with inflammation of the chest, and died. Mr Burnam, who attended him at Wisbeach, wrote me word that he had recovered the use of the limb, and that the swelling in the ham had almost disappeared before his death.

In this case the origin of the disease in the popliteal artery was clearly traceable to rheumatism. It is very seldom that such a cause can be assigned for the commencement of aneurisms in the extremities; the disease being usually attributed to an injury, a sprain, or to free movement of the part. Even in the thorax the diseases of the arterial coats that give rise to aneurism are more generally dependent upon age or intemperance than rheumatism.

The extent to which the characteristic symptoms of aneurism were, on the patient's admission, masked by a tense inflammatory condition of the surrounding tissues, deserves particular attention. At one time both pulsation and bruit were entirely absent, and pressure upon the femoral artery failed to produce any alteration in the swelling; indeed the swelling then resembled an abscess more than an aneurism, and it is certainly no wonder that mistakes in diagnosis should, under similar circumstances, have, in more than one case, led to serious mistakes in treatment. As the inflammation and tension diminished, first the bruit and then the pulsation returned.

The cure by pressure upon the artery above the aneurism was effected in a shorter space of time than in any other case of which I have seen an account; all pulsation in the tumour having ceased fifteen hours after the treatment was commenced, and it never returned.* A case is related in Dr Bellingham's "*Observations on Aneurism*," treated at the Royal Military Infirmary, Dublin, in which pulsation ceased after ten hours of compression, maintained by two instruments alternately relaxed, (the pressure upon the artery being sufficient to check completely the pulsation in the tumour,) and it did not return. In this case, however, it is to be observed, that pressure had previously been applied to the artery in the groin by a weight, and had, it was supposed, caused some deposition of fibrin in the aneurismal sac. Not long ago a case was related at the Medico-Chirurgical Society, by Dr Bellingham, in which the pulsation had ceased twenty-three hours after compression had been commenced. In the greater number of cases of which reports have been published, weeks, and even months, of patient endurance and constant attention have been required to bring about a successful issue. Indeed the great, perhaps the only, valid objection to the treatment of aneurisms by compression of the artery above the tumour, is founded on the tediousness of the process, and the pain to which the patient is subjected

* It has been already shown in this Journal that the shortest time for the cure by compression took place in the case by Dr Hutton after seven hours; and in one case by Dr Humphrey, after ten hours.—*Edinburgh Medical and Surgical Journal*, Vol. lxxvi., p. 232. July 1851.

tained that a small segment of the head could with some difficulty be reached, just sufficient to enable the perforator to be used; and the majority of the accoucheurs considering the possibility of perforation, and the great success that had attended the former deliveries as regarded herself, were of opinion that the head should be lessened, and time given to try how far the uterine efforts, which were now vigorous, might mould the collapsed bones to the space through which they had to pass. Feeling the great responsibility that would attach to me individually, if he acted in accordance with my own views of the case, in opposition to the judgment of such sound and experienced practitioners, and failed in an operation which they deemed ineligible, Dr Shekleton determined on yielding to their decision, and undertook the duty of carrying it into effect. Accordingly, he introduced the perforator, and made, as far as the space would allow the handles to diverge, the ordinary crucial incision. Judging that the crotchet would enable him to use more freedom in breaking up the cerebral mass, he substituted it for the perforator, and in a short time evacuated a large portion of the contents of the cranium. Five o'clock having now arrived, the attendants adjourned till 7, when Drs McClinton and Denham, who assisted Dr Johnson at the last delivery, and knew all the particulars of the case, afforded their aid, in addition to Dr Hardy and Drs Sibthorpe and Johnston.

On examination now Dr Shekleton found that the left arm of the child had fallen into the narrow space in the vagina, and no effort could return it, nor could a finger be passed up at the side of it to ascertain the position of the head; it was, therefore, evident that the case had become seriously complicated by this untoward change of presentation, and that no alternative was left but to separate the arm from the body, eviscerate the child, and deliver as best we could with the crotchet. Accordingly the instrument was fixed in the axilla, and such traction employed, that at length the arm gave way, bringing with it the scapula of that side. By degrees the whole of the thoracic viscera, the ribs, and the contents of the abdomen, were torn away, and finally the spine was unintentionally divided in the middle, and many of the vertebrae were extracted separately. After various attempts and failures two crotchets were at length firmly fixed in the foetal pelvis, and by the united alternate efforts of the operators, they at length succeeded in dragging the lower extremities through the os externum, and with them the right arm, attached by a strip of integument and torn muscles. In this stage of the operation the funis got entangled in the crotchet, and the placenta came away with it in the efforts to extract the extremities, but no hemorrhage ensued. A most perplexing and difficult operation at all times, but especially so in the present instance, still remained to be performed, namely, the extraction of the child's head. In despair of finding any means by which to fix it at the brim, and grasp it by the crotchet Dr. Shekleton introduced his fingers into the vagina, and there found the stump of the dorsal vertebrae, with small portions of ribs attached, which he instantly secured, and firmly held till the crotchet was fixed at the back of the ear externally, and the head extracted by Dr. Denham with extreme difficulty, and in a very flattened form.

Thus ended the mechanical efforts by which delivery was at length effected, which occupied upwards of three hours at the last sitting.

Till within about twenty minutes of the final step in the operation, the pains appeared strong and energetic, aiding the efforts at delivery, but a great change was now manifest in the sufferer; vomiting set in; the pulse became weak and rapid; brandy and other stimulants were freely administered, without effect; the surface was bedewed with

cold perspiration; she ceased to complain, except by subdued moans and sighs; in short, death was depicted in her countenance; and in ten minutes after the head was extracted, Anne Parsons breathed her last.

Autopsy.—Eleven hours after the scene thus detailed an examination of the body was made, and the causes of delay and death were clearly and satisfactorily presented. The uterus was lying at the right side of the abdomen, with its fundus reaching a little above the umbilicus, giving it the appearance of unusual bulk, but this was evidently owing to the entire organ being in the abdomen, instead of its lower third having sunk into the pelvic cavity, as in the ordinary state, immediately after parturition. The structure of the body was firm and thick, but towards the cervix and as it appeared flattened and much thinned in its structure, and was projected forwards by the fundus of the tumour which occupied the left iliac fossa. The Fallopian tubes and ovaries at either side were healthy. The bladder was small and closely contracted. The peritoneal covering of the anterior abdominal parietes was much injected; there were, however, no other signs of inflammation, if this could properly be called one. On lifting the fundus of the uterus forwards, a quantity of blood, with small portions of foetal brain, was seen. This, no doubt, escaped from the perforation in the cranium in the first stage of the delivery, and ultimately made its way into the cavity of the peritoneum through an extensive laceration which was now discovered. This laceration occupied the posterior wall of the uterus, was about five inches long, running obliquely from the right downwards through the cervix uteri and upper part of the vagina; involving the vagina, however, in the largest portion of its extent. Blood was extravasated between the peritoneum and posterior abdominal walls. The rent in the peritoneum covering the uterus was rather more extensive than that of the structure of the uterus itself, from which, to some extent, the peritoneal tunic was separated by coagula. Thus far as concerns the immediate cause of death.

The tumour was a large firm mass, nearly filling up the entire aperture of the true pelvis, except a very small space on the right side, which space was evidently much enlarged by the great pressure to which the tumour was so long subjected during the operation. It was of a pyriform shape, and extended from near the point of the coccyx to the superior brim of the false pelvis. It was found to be immoveably fixed in its situation, being firmly united to the periosteum (except at its fundus and superior part of its anterior wall) and to some of the last sacral nerves, one of which appeared to enter into, and to be lost in its substance. A portion of the anterior wall of the sacrum had been absorbed, and a part of the tumour had extended into the sacral canal, and lay anterior to the cauda equina. The rectum was pushed completely out of its natural position, and lay in the right side of the pelvis, free from pressure. On cutting into the tumour directly from above downwards, it was found to be very fibrous and solid throughout, except at its fundus, where there was a small cyst, lined by a fine vascular membrane, and containing about four drachms of a reddish serous-looking fluid. This cyst, as well as a smaller one, containing about one drachm, near its apex, was not discovered till after death; every part of the tumour within reach, feeling so firm and resisting to the touch, that it was thought by all to be of bony origin and structure.

The chief object of Dr Shekleton in bringing this case before the Society was to elicit an opinion as to the propriety of having recourse to the Cæsarean section, in cases similarly circumstanced with the one now related. It is to be regretted that the unsuccessful results that attended the operation for many years had so enslaved men's energies and judg-

necessity, but of election, and therefore they operated earlier, and under more favourable circumstances, no doubt often unnecessarily, as we could show.

Dr Churchill has collected from foreign authorities 371 cases of Cæsarean section, out of which 217 mothers recovered from the operation, and 154 died; but perhaps not all of these from the operation itself. Out of 189 of these cases, where the results to the children were given, 139 were saved, and only 50 lost.

Now, out of fifty-two cases reported by British and American practitioners (as recorded in the second edition of Dr Churchill's *Practice of Midwifery*), fourteen mothers only were saved, and thirty-eight died. This difference in mortality can only be accounted for by the delay in the operation, for most of the unsuccessful cases were days in labour:—one twelve days; others ten, eight, seven, three, &c.

He further gives from the work of Dr Meigs a case of difficult labour from deformed pelvis, occurring in a lady with her first child.

The deformity of the upper strait of the pelvis was so great (barely two inches in its antero-posterior diameter), that a living child could not possibly pass through it. A consultation was had, and it was agreed to wait some hours to see what effect uterine efforts might have upon its progress. At length it was decided that something should be done: the child was alive and vigorous. "The different methods of proceeding which have been proposed in similar cases were considered, namely, the division of the symphysis pubis, the Cæsarean section, and cephalotomy. The first was deemed inapplicable. The Cæsarean section was thought to be attended with so much risk to the mother as almost to be necessarily fatal, and some of the most distinguished medical men, including Dr Physic, were decidedly opposed to its performance.

Dr Meigs states that both he and Dr. Lukens were of opinion that the operation of cephalotomy, if not altogether incompetent to the delivery, would be attended with as much risk to the life of the mother as the Cæsarean section, for it appeared to them impossible that the cranium could be brought through the superior strait without the most violent exertions and great danger of lacerating the cervix uteri and vagina. Cephalotomy, however, was with great difficulty performed, and like Elizabeth Sherwood, the patient recovered, and was about her usual occupations in three weeks. In two years after, this same patient again fell under Dr Meigs' care at the full term, having resisted the advice both of himself and Dr Dewees to allow premature delivery to be induced at the proper time, and now refusing to submit to the Cæsarean section, and again craniotomy was performed. He does not say how long the operation lasted in this instance, but it was of many hours' duration in the first. It fell to Dr. Nancrede's lot to have the care of this lady in her third confinement. The breech presented, and delivery "*per vias naturales*" was deemed impossible; but she was safely delivered of a living child by means of the Cæsarean operation, performed by Professor Gibson. In a subsequent or fourth pregnancy, the same operation was a second time performed successfully by Dr. Gibson, in presence of Dr. Meigs and others, and both mother and child were again happily preserved. Dr Shekleton relates this case briefly merely to show, that in this instance the Cæsarean section was twice happily performed as a matter of *election*, not of necessity, for it does not appear that the impediment to her two first deliveries had increased, but her children had to be sacrificed; and the lady, like unfortunate Anne Parsons, would, in all probability, have ultimately fallen a victim to the repeated employment of the perforator and crotchet.

- ART. II.—1. Practical Observations on Mineral Waters and Baths, with Notices of some Continental Climates, and a Reprint, the Third of the Cold Water Cure. By EDWIN LEE, Esq.
 2. Bradshaw's Companion to the Continent. A Descriptive Handbook to the Chief Places of Resort ; their Characteristic Features, Climate, Scenery, and Remedial Resources ; with Observations on the Influence of Climate and Travelling. By EDWIN LEE, Esq.
 3. Lectures on the German Mineral Waters, and on their Rational Employment for the Cure of Certain Chronic Diseases. By SIGISMUND SUTRO, M. D., . . . Page 451

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ERRATA ET CORRIGENDA.

Page 2d, after Lancet, vol. i., insert 1833-1834.

— 7th, 11th line from top, for "wrists," read "palma."

— 224, for "Denman," read "Denham."

— 380, 17th line from top, for "opral," read "spinal."

NOTICES TO CORRESPONDENTS.

The following works have been received :—

Medical Lexicon : A Dictionary of Medical Science, containing a concise explanation of the various subjects and terms of Physiology, Pathology, Hygiene, Therapeutics, Pharmacology. Obstetrics, Medical Jurisprudence, &c.; with the French and other Synonyms; notices of Climate and of celebrated Mineral Waters, Formulæ for various Official, Empirical, and Dietetic Preparations, &c. By Robley Dunglison, M.D., Professor of the Institutes of Medicine, etc., in Jefferson Medical College, Philadelphia. Eight edition, revised and greatly enlarged. Philadelphia, 1851. Large 8vo. Pp. 927.

The Board of Health and the Cholera. A Memoir on the Saline Treatment. Worcester, 1852. Foolscep 8vo. Pp. 227.

Report of the Proceedings of the Pathological Society of London. Fifth Session, 1851-1852. London. 8vo. Pp. 196

Medicina Mechanica, or the Theory and Practice of Active and Passive Exercises and Manipulations; considered as a branch of Therapeutics, and as adapted both to the Treatment and cure of many forms of Chronic Disease. By John W. F. Blundell, M.D. London, 1852. Post 8vo. Pp. 292.

Report of the Manchester Royal Lunatic Hospital, situate at Cheadle, Cheshire, in connection with the Manchester Royal Infirmary, from June 25th 1850, to June 24th 1851. Manchester, 1852. 8vo. Pp. 19.

Notes on Bright's Disease of the Kidney, as observed chiefly in the Clinical Ward of the Jam-etjee Jejeeboy Hospital at Bombay. By C. Morehead, M.D., Professor of Medicine, Grant Medical College. (Bombay Transactions, No. x.) 8vo. Pp. 46.

Homœopathy in 1851. Edited by J. Rutherford Russell, M.D. Edinburgh, 1852. 12mo. Pp. 116.

The Fallacies of Homœopathy, and the Imperfect Statistical Inquiries in which the results of that Practice are estimated. By C. H. F. Routh, M.D., M.R.C.S., Physician to the Saint Pancras Royal General Dispensary, &c. &c. London, 1852. 8vo. Pp. 85.

Researches and Observations on Scrofulous Disease of the External Lymphatic Glands. With Cases showing its connection with Pulmonary Consumption and other Diseases. By Thomas Balman, M.D., M.R.C.S., and L.S.A., &c., one of the Medical Officers of St Ann's Dispensary, Liverpool. London, 1852. 8vo. Pp. 189.

Physiology applied to Health and the Improvement of Physical and Mental Education. By Andrew Combe, M.D., Fellow of the R. College of Physicians of Edinburgh; Physician Extraordinary in Scotland to the Queen. Fourteenth Edition. Revised and Enlarged. Edited by James Cox, M.D., Fellow of the Royal College of Physicians of Edinburgh. Edinburgh, 1852. Post 8vo. Pp. 345.

Sketches of Brazil; including New Views of Tropical and European Fever, with Remarks on a Premature Decay of the System incident to Europeans on their Return from Hot Climates. By Robert Dundas, M.D., Physician to the Northern Hospital, Liverpool; formerly Acting Surgeon to H.M. 60th Regiment; and for twenty-three years Medical Superintendent of the British Hospitals, Bahia. London, 1852. Post 8vo. Pp. 449.

Lateral Curvature of the Spine, its Causes, Nature, and Treatment. By R. W. Tamplin, F.R.C.S.E.; Surgeon to and Lecturer on Deformities at the Royal Orthopædic Hospital, Bloomsbury Square, &c. London, 1852. Large 8vo. Pp. 44. Four Lithograph Figures.

The Stomach and its Difficulties. By Sir James Eyre, M.D., Edinburgh; Licentiate of the Royal College of Physicians of London; Consulting Phy-

THE
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1ST APRIL 1852.

PART I.
ORIGINAL COMMUNICATIONS.

ART. I.—On Disease of the Pituitary Gland. By ROBERT SCOTT ORR, M.D., formerly Resident Superintendant of the Glasgow Royal Infirmary.

IN the last Number of this Journal I reported a very interesting case of epilepsy, in which, among numerous other morbid appearances observed after death, the pituitary gland presented distinct traces of diseased action. Since the occurrence of that case, now more than ten years ago, my attention has always been directed, in every succeeding *post mortem* examination, where an opportunity of examining the head was permitted, to the state and appearance of the pituitary body, and I have neglected no opportunity of observing it with care and attention.

In a few rare instances, I have found it presenting various marks of disease. It is probable, however, that it is but seldom subject to morbid changes; at the same time, it is to be feared, that it is often overlooked in post mortem examinations. The attention of the dissector is generally given entirely to the appearances found in the other contents of the cranium, and as the pituitary gland cannot easily be removed along with the brain, it is either forgotten altogether, or neglected to be removed afterwards for examination. That this is the case is highly probable, from the fact, that it is scarcely ever noticed in reports of dissections, and in searching for cases illustrative of its diseases and pathology, I have found but little written on the subject.

The function of the pituitary body being entirely unknown to us, and its pathology having been hitherto so little studied, it is with the view of calling attention to the subject that I venture to

submit the following remarks and cases. The subject is confessedly an obscure one. I shall not therefore indulge in any theoretical speculations, but adhere to carefully recorded facts, trusting that though these are somewhat meagre and scanty, they may not prove uninteresting.

With the exception of those recorded in the treatise of the brothers Wenzel on Epilepsy,* I have been able to find only one detailed case of disease of the pituitary body. They give an account of twenty cases, where the dissections appear to have been conducted with great care and minuteness, and in all these more or less disease was discovered in this body. All the patients had been distinctly epileptic; but as I do not mean to enter into the question as to the connection between this disease and the morbid lesions found, I content myself for the present with remarking that the coincidence is curious, and shall allude to them merely as specimens of the morbid anatomy of the structure we are considering.

Rokitansky,† in his work on Pathological Anatomy, divides the diseases of the pituitary gland that he has observed into three heads. Under the first head he comprises anomalies of size,—under the second, diseases of texture, and under the third adventitious growths. I shall attempt to follow him in this arrangement.

1. *Anomalies in size.*—"Increase," he says, "in the volume of the pituitary gland, may occur from congestion, inflammatory swelling and abscess, or from the presence of an adventitious growth, but he has never observed any actual hypertrophy of the gland."

Atrophy he has frequently found. "It accompanies in old persons, the same affections of the brain. It prevails especially in the posterior lobe, which, besides diminishing in size, becomes loosened and pulpy, and discoloured to rusty brown or yeast yellow. At the same time the anterior lobe becomes pale, withered, and tough. Moreover, atrophy is sometimes produced, in cases of chronic dropsy of the ventricles, by the pressure which is exerted upon the gland by the serous effusions, through the medium of the third ventricle.

"The *infundibulum* in like manner usually shares in the atrophy, and becomes pale, withered, and thin."

1. A few weeks ago, I met with a case of hydrocephalus in a female child of seven years of age, in which, after death, the appearances last described above were extremely well marked,

* Translated by M. Breton. Paris, 1811.

† Rokitansky's Pathol. Anat. Trans. for the Sydenham Society, by Chas. H. Moore. Vol. iii. page 432.

and as the case was altogether an interesting one, I shall make no apology for detailing it somewhat at length.

The patient had from infancy been always a delicate child, and complained more or less of her head. Dentition was, however, effected with tolerable ease. She had been ill eight days before I was called in to see her. The prominent symptoms were pain in the head, and constant vomiting when food was attempted to be taken. I was sent for in consequence of a restless night with aggravation of all her symptoms, the supervention of deafness, and considerable stupidity of manner and deportment.

On examination I found her free from fever. Her pulse was 82; tongue pretty clean and moist; bowels reported slow. She was exceedingly restless and talked incoherently, laughing when spoken to with an imbecile expression of countenance. She still continued deaf. Her head was cool, pupils were much dilated, and the vomiting was reported to continue.

The head was shaved and a blister applied behind the left ear over the parietal bone. A grain and a-half of calomel was directed to be given every four hours, and three drops of the tincture of iodine in water three times a-day.

In the evening the pulse rose to 120. The skin felt more hot and feverish than in the morning. The bowels had acted freely. She spent a restless night with much incoherent talking.

On the 10th day of her illness her pulse was 104, soft and regular, skin cool, pupils dilated, tongue clean; she laughed idiotically when spoken to, and could not answer questions.

The blistered surface was directed to be dressed with equal parts of mercurial and iodine ointment.

At 6 o'clock in the evening I was sent for in consequence of the sudden supervention of coma, which occurred immediately after an attack of vomiting. I found her quite comatose and apparently near death. Her pulse was 64, irregular; pupils much dilated. An enema with half an ounce of turpentine was instantly administered, and mustard sinapisms were applied to the calves of the legs, after which she almost immediately began to revive, and when roused could say she felt better; the pulse now rose to 84.

A blister was applied over the right parietal bone and behind the ear.

At half-past 10 P.M., she again became comatose, but could with difficulty be roused, and answered "yes and no" to questions. The pulse was 68, irregular, pupils contracted and insensible to the light of a candle. The turpentine enema was still retained. The head was cool, and as yet no convulsions had occurred.

The calomel was directed to be continued, and the turpentine enema to be repeated in two hours.

On the 11th day she still continued drowsy, but could be roused, and protruded the tongue when desired. She vomited three times during the night, and the bowels acted once. Pupils much dilated. Pulse 72, very small and feeble.

Both blisters were directed to be dressed with the mercurial and iodine ointment, and another blister was applied to the vertex.

In the evening, though she still continued drowsy, she had occasional intervals of consciousness, when she revived, looked about her, and answered "yes and no" to questions, vomiting continued unabated, pupils very much dilated. Pulse 64, of better strength, but still languid.

She continued much in the same state during the 12th day.

On the 13th day drowsiness continued. Slight convulsions commenced in the morning. When I visited her in the forenoon I found the arms and hands tremulous from convulsive spasms; they could not be flexed without considerable force being used, in consequence of rigidity of the muscles.

A blister was again applied behind the left ear, and the calomel was repeated.

In the evening she was quite comatose, the muscles of the arms were still rigid. It was evident she was fast sinking, and at half-past nine o'clock she died.

The following appearances were observed on dissection thirty-eight hours after death.

Head.—On removing the skull-cap, very strong adhesions were observed between the *dura mater* and calvarium, extending throughout the anterior half of its inner surface. The surface of the brain was unusually dry, without the least appearance of effusion.

On making a section of the brain, about three or four ounces of clear serum escaped from the lateral ventricles. The anterior part of the *fornix* and *septum lucidum* was softened. The choroid plexus was blanched and bloodless. There was a recent effusion of soft flaky lymph over the *Pons Varolii* and *medulla oblongata*. A considerable quantity of fluid escaped from the spinal cavity.

From six to eight hard yellow tubercles were found imbedded in the substance of the *cerebellum*, one of which was as large as a barleycorn. A single tubercle was also found in the *Pons Varolii* (a rare occurrence, Rokitsansky, page 428), and another in the left hemisphere of the *cerebrum*.

The pituitary gland was unusually small, and did not seem to fill as it ought its cavity in the *sella turcica*. The *infundibulum* was small and filiform, and appeared to be blanched and bloodless; in short, both it and the pituitary gland were evidently much atrophied. The posterior clinoid processes of the sphenoid

bone were thicker than usual, and were bent much forward, overlapping the gland.

Chest.—Both lungs were studded from the *apices* to the inferior borders with miliary tubercles. The right lung adhered to the upper part of the parietes of the chest.

There was some fluid within the pericardium. The heart was normal.

Abdomen.—The mesentery was studded with tubercles, varying in size from a horse bean to a pinhead. The liver and other organs were healthy.

About a fortnight after witnessing this inspection, I had an opportunity of contrasting the appearances in the pituitary gland and clinoid processes in this case, with those found in another female child of four years old, who died of head disease, and in whom these structures were quite healthy. In this case, though the child was three years younger, the pituitary gland was at least a third larger than in the former case; it was healthy in its structure and filled its cavity properly; the *infundibulum* was of the usual tubular funnel-shape and size, and of its natural pink colour; there was no thickening of the clinoid processes, and they were not bent forward as in the former case. I mention these circumstances to show, that in judging of the morbid appearances described, I have not trusted to my own recollection as to what should be the natural appearance of the structures under consideration, but have tested them by actual comparison with a healthy specimen.

In the first, the sixth, and sixteenth cases detailed by the Wenzels, the pituitary gland seems to have been in a state of atrophy.

In the sixth case it resembled a vesicle; in short, it wanted a small part of the posterior border of the great lobe and the greatest part of the small lobe. The place where this loss of substance had taken place was filled with the peculiar yellow matter to be afterwards mentioned.

In the sixteenth case the posterior lobe was small, shortened, and thickened. When cut, the two portions exhibited two places of dazzling whiteness and cartilaginous appearance. On raising a small fragment with a scalpel, a thick milky fluid flowed out.

These observations agree with those of Rokitsansky as to the posterior lobe being most frequently found atrophied.

In their eighteenth case the Wenzels found the posterior apophyses extremely bent, and the pommel of the *sella turcica* very thick. This bending forward of the clinoid processes was very remarkable in the case of the little girl which I have just related.

Morgagni found the gland depressed, small, and extremely soft. Grading notes it as small in four persons out of twelve.

2. *Diseases of Texture*.—Under this head Rokitansky places first Hyperæmia and Anæmia.

The fourth and eighth cases given by the Wenzels appear to have been examples of *hyperæmia*. In the former the gland was of monstrous size, greatly distended, the superior surface of a deep-red colour, and all the vessels were distended with blood. The posterior lobe, which he says in adults is always grey, was of a deep-blue colour. In the latter the anterior lobe externally was somewhat white; when cut horizontally the middle was of a deep-red colour verging to black. The posterior lobe was of a grey colour within and without.

Rokitansky says, "Congestion of the pituitary gland is generally combined with the like condition of the *pia mater*; but in young persons it is occasionally observed alone. It occupies the anterior vascular lobe, and the *infundibulum*. The cluster of vessels descending on either side of the latter to the gland is injected, the substance of the *infundibulum* is redder than natural, and the turgid gland is loose in its texture, dark-red, and full of blood. In some very rare instances the congestion leads to apoplectic extravasation. I have observed small streaks of it associated with congestion of the membranes."

"The gland is anæmic in cases of anæmia of the membranes."

The distinction between congestion and the first stage of inflammation in an obscure structure like that of the pituitary gland cannot, it is probable, be made with great minuteness. I pass on to the second division under diseases of texture, viz.

Inflammation.—It is here interesting to observe how exactly the description given by Rokitansky of the inflammatory appearances, he observed, agrees with those found in the cases recorded by the Wenzels. Thus both agree in finding the anterior lobe most frequently inflamed, both agree in their descriptions of the swelling, dark reddening, and looseness of the texture of the gland, and also in the lymphic exudations they observed.

In their ninth case the Wenzels found the pituitary gland remarkable for its clearness and extraordinary redness; the anterior lobe presented all the appearance of inflammation. The posterior lobe was white, whilst at the age (24 years) of the patient from whom it was taken, it is always grey. On the borders of the anterior lobe was a bright-red substance, and within a white substance, which filled almost all the capacity of the superior part.

In their tenth case there was found between the lobes of the gland a thick white matter greatly resembling congealed and hardened lymph.

The marks of inflammation in their eleventh case were very dis-

bathing his forehead with cold diluted vinegar, without benefit, he sought medical advice. He was then treated for bilious headache, with some simple medicines, and in a few days the pain was very much relieved, and the slight concurrent feverish symptoms disappeared.

He began at this period to examine his business papers, and committed some dietetic errors. These imprudences were followed by a return of the pain with renewed force. He was at the same time excited, restless, and talked wildly; or, on the other hand, he would lie in a state of semi-stupor, silent, and absolutely indifferent to every thing around him. His pulse became quick, and he lost all appetite. Still his attendant thought the case one of no great importance,—a kind of stomach derangement. In health he had a jocose, wayward, and half mischievous way about him; and it must be confessed, his wild talking appeared very easily to be only an exaggeration of his ordinary manner. In the midst of all this,—four or five mornings after the renewed attack,—he stripped himself perfectly naked, jumped out of bed, and passed a stool on the floor while standing before his attendant and the physician.

Consultations were held, mercury given vigorously, leeches and blisters applied. Nothing gave a decided check to the progress of the disease, and he fell into a state of profound coma. The pulse became intermittent and very slow, some days even below 40 in the minute. It was now believed that he was in fever. Matters certainly came to a *crisis* on the evening of about the eleventh day after the supervention of distinct symptoms. His pulse became almost imperceptible,—extremities cold,—hands convulsively clenched,—jaws so firmly closed that, but for the cavity left by the loss of a tooth, nothing could have been poured down the throat. Wine was given freely, yet with caution; another blister was applied to the back of the neck; and from this day a gradual improvement took place.

This improvement was ascribed to the mercury, which had commenced to affect the mouth, and the other therapeutic means which had been employed.

From this period commences the knowledge of the case by Dr Heslop.

With careful support and stimulation he rapidly rallied, and, though occasionally retrograding and showing some of his former bad symptoms, twelve days afterwards he was so rational and had so good an appetite, that Dr Heslop confidently predicted an early and complete recovery. Yet two symptoms remained which caused Dr Heslop some misgivings. One of these was an almost total loss of memory; a few minutes after taking a glass of

wine he would utterly forget having had it. No evidence could induce him to believe he was in Ireland and not in Sussex, where he had spent a few days two or three years before. The other ill-ominous symptom was loss of power over the sphincters. Now and then he would give signs that he wished to pass a stool, but the rule was, that it was voided unconsciously and without warning; and when he occasionally did ask for the urinal, it was almost certain that he would pass water before the attendant could give the utensil to him, so sudden and urgent was the impulse to evacuate the contents of the bladder.

He was nevertheless deemed sufficiently well a fortnight afterwards to undertake a journey of sixty miles to Dublin; upon arriving in which city he was put under the care of Dr Stokes. It would be tedious to go into anything like a detailed description of the many phases of a chronic and ever-varying disease. It may suffice to state, that he soon altogether lost control over the sphincters, that he progressively became emaciated, and that his memory appeared to have almost vanished; still, when tolerably well, he retained a considerable amount of intelligence, and frequently answered questions quite coherently, though so unwilling to speak that whole days used to pass without his saying more than "yes" or "no" occasionally. The idea irresistibly conveyed to the mind was, that, had the single faculty of memory been perfect, the intellect would have been sound. With the exception already given, he had paralysis of only one muscle—the orbicularis of the right eye-lid, which for several months before his death he did not close, sleeping or waking. The acute pains in the head never forsook him. Sometimes several days passed over without these attacks, but they were rarely more than two days apart; these always came on suddenly, when the face instantly became of a scarlet hue. While strong enough, he raised his hands to the head and gave a look, and uttered groans, too distinctly portraying the agony of the moment. The paroxysm rarely lasted more than ten or fifteen minutes, and was always succeeded by a profound stupor of some hours duration. At various periods he suffered also from severe cramps of the lower extremities. Towards the end of August it became obvious that the strength was nearly exhausted; but he was destined to give another illustration of the law that chronic disease kills by giving the predisposition to an acute one. An obscure attack of pleurisy came on, of which he died on the 4th of September.

Inspection thirty Hours after Death.—The body was extremely emaciated. Upon taking off the calvarium the membranes covering the hemispheres of the brain appeared quite healthy; the inspector detached the brain from the base of the skull; when he arrived at the second pair of nerves, he found them almost

surrounded by a softish, dark grey tumour, about the size of a large walnut, occupying the position of the pituitary body on the *sella turcica*, but extending much beyond the space usually held by that body both posteriorly and laterally. It was very carefully dissected from the *dura mater* and surrounding structures, but not without slightly lacerating it and giving exit to a small quantity of thick, dark red, purulent looking fluid.

Having taken the brain out of the skull, and turned its base upwards, the exact relations of the tumour were examined. In this position it filled the entire interpeduncular space, extending backwards to the *Pons Varolii*, forwards and laterally protruding into the commencement of the fissure of Sylvius; the *crura cerebri* were distinctly pressed *outwards*, so that the space just referred to was greater than natural; it could be easily raised from its position after dissecting off a few delicate cellular bands, having no other connection with the brain than through the pituitary body, which seemed lost in the tumour; or rather this latter seemed a *cystiform enlargement* of that body containing the purulent-looking fluid already described. Underneath the tumour was observed a small quantity of thin fluid, of a slightly reddish tinge, upon drawing off which it was remarked, that it was but the surface of a more deeply situated fluid, contained in the lateral ventricles. Twelve or fourteen drachms were measured, but probably an equal quantity lay on the base of the skull, of which no note was taken; in fact, the tumour occupied not merely the floor, but almost the entire cavity of the third ventricle; the anatomical relations of the former were precisely identical with those of the latter.

The quadrigeminal bodies, the inspector thought, seemed flattened in the antero-posterior direction by the pressure of the tumour.

When we add an increased firmness on section of the *medulla oblongata*, and slight opacity, with thickening of the arachnoid, over the middle lobes of the base of the brain, we have mentioned all the morbid appearances in the encephalon. But we must record the fact, that the bones entering into the formation of the middle fossa of the base of the skull had scattered over various parts of them, curious little stalactitic processes or exostoses, varying from one to three or four lines in length; the elevations, too, corresponding to the sulci of the middle lobes of the brain, were extremely sharp and angular.

Dr Heslop declines inquiring whether these had any, or what share in the production of the remarkable disease under which this gentleman suffered.

No tubercle could be found in the lungs. There was a considerable amount of recent lymph over the base of the left lung and corresponding surface of the diaphragm.

Stomach.—The inner membrane of the stomach was very pale, and in some places much stained with bile, but nothing was found to account for the vomiting during life.

Liver.—The liver was light coloured, hard and brittle, and about the natural size; spleen healthy.

Kidneys.—Both kidneys were somewhat smaller than usual. A cyst capable of holding a teaspoonful of fluid occupied the centre of the right kidney. In other respects both kidneys were quite healthy.

In this case the morbid growth was the only example of cancerous disease found in the body. It could not be ascertained whether the patient had ever been subject to fits of any kind, but it was known that she was weak minded.

In more than one of the cases described by the Wenzels the gland was of a scirrhous cartilaginous hardness; and they mention that Petit in a hydrocephalic case found it excessively voluminous, and of a scirrhous consistence.

3. The most singular of all the adventitious productions noticed as occurring in connection with the pituitary gland remains now to be considered. This consists in the effusion of a matter varying from a semifluid consistence to that of a gelatiniform or flaky hardness of various shades of colour, verging from grey to greyish-yellow, yellow, and deep amber. It is generally found underneath the gland and between its lobes. It is described by Rokitsky as "a gummy glutinous (colloid) substance, of a yellow colour, like citron or yellow wine, and occupies the cellular interstice between the two lobes of the gland." The Wenzels describe it as a yellow, viscid, transparent, friable matter, separating the two lobes.

This appears to be the most common morbid appearance in connection with the pituitary gland, at least it has more frequently been found than any other form of disease, and the descriptions given of it are remarkable for their uniformity. The Wenzels found it remarkably distinct in ten of their cases.

Is it an inflammatory product? This is not an improbable supposition, for in several of the Wenzels' cases there was found in the same situation where this substance was deposited, effusions of lymph of various degrees of consistency, and in various transition stages, from that of complete fluidity to that of solid lymph, as well as other traces of inflammatory action. In Duncan's case the inflammatory appearances were abundantly evident, and the amber-coloured body found underneath the pituitary gland formed the central nucleus of a yellow, glutinous, transparent mass lying underneath and between the lobes. Her case exactly cor-

Physicians accordingly substituted in place of the theory of Galen upon the liver and its functions, the discoveries made in the seventeenth century; and by these they believed that it became more easy to comprehend the phenomena of life and the causes of diseases. Among the latter subjects, there is in particular one disease, the pathogeny of which appears quite simple and natural according to the new doctrines. This is dropsy. The formation of this disease, Galen, it is well known, explained by the assumption of a cold and moist state of the liver. Several physicians believed that the cause of this disease consisted in rupture of the lymphatic vessels and effusion of lymph. Lower meanwhile opposed this manner of explanation, as one which is purely imaginary. He was the first who demonstrated the proposition, that obstruction of the veins is capable of producing dropsy; and this he did by the aid of a decisive experiment, that of enclosing the venous trunks in a ligature.*

Further, one hundred years after the time of Lower, Van Swieten, commenting upon this experimental fact of Lower, remarks, that the ancients had some reason in regarding the diseased liver as the cause of all dropsies; because this organ, when it is changed by disease, must of necessity obstruct the venous circulation, either of the *vena cava*, which traverses it, or of the portal vein, which

splenic parenchyma, that is to say, of the spleen or of the liver; for parenchyma means both the one and the other organ, by reason of the close sympathy which subsists between them through the medium of the *vas breve* of the *pylorus*.

His physicians, as thus strongly imbued with Galenical doctrines, Moliere represents, in order to cover them with ridicule, as strictly opposed to the doctrines of Harvey and of Pecquet. "I have," says one, "maintained a thesis against the Circulators." The same person, at the saying of his father, who is like himself an obstinate partisan of the ancient medicine, "is blindly attached to the opinions of the ancients, and he never desired either to listen to, or to comprehend the reasons and the experiments of the alleged discoveries of our age (the seventeenth century) regarding the circulation of the blood and the other opinions of the same stamp;" that is to say, the opinions of Pecquet and of Bartholin, which, as has been above said, constituted part of the same body with that of Harvey.

By this it appears that Moliere attacked medicine in general less than is commonly believed. He merely took part in the discussion, which was at that time going on, between the Galenical physicians on the one hand, represented by Riolan, and Bartholin on the other side, who was maintaining the new ideas. He had reason to attack Galenism, in whatever false and ridiculous doctrines it really contained, and especially the black bile of the spleen and its supposed deleterious influence.

It is well known that Moliere received his information as to the facts and the style of the medicine of the day from Mauvilain, professor of the faculty.

* Richard Lower is one of the most original and the most acute authors of the seventeenth century. He has, in particular, shown all the pathological consequences of the discovery of Harvey, in a small work of about two hundred pages, full of facts and judicious reflections (*Tractatus de Corde Amstelodami, 1671*). In this author we find a theory of the uses of the pancreatic juice, which approaches in some degree to that given by M. Bernard—"Cum autem chylus per hujusmodi poros et angustias ex intestinis in lacteas transeat, a ratione non alienum videtur, ab humore e pancreate in duodenum secreto, pro faciliore transitu magis dilui, et attenuari." *Ibid.* p. 202.

presence of gall stone.* We cannot indeed deny the existence of hepatalgic attacks produced by a gall-stone; but these are not common. In conclusion, it must be admitted, that, in the great majority of cases, neuralgic attacks in the liver proceed from other causes than calculous obstruction.

What then, it may be asked, are the causes of hepatalgia other than the presence of a gall-stone in the biliary passages?

It may be said *a priori*, that these are all the causes of visceral neuralgias in general. In this sense it is necessary to make researches in order to elucidate the etiology of hepatic colic; I doubt not that there are observed syphilitic, gouty, and rheumatic hepatalgias. One of the last species I have already met with. The individual in whom it took place, who was subject to rheumatic ailments, was without doubt affected by hepatic colic, which was distinctly marked, whenever he took not care to clothe himself warmly, especially at the period of the first cold weather; and during the whole time that the acute or dull pains in the liver continued he was exempt from every other species of rheumatic disorder.

There is one cause of hepatalgia, which to me appears to deserve particular attention, as connecting itself in an intimate manner with the subject of this memoir. It is the cause which resides in certain articles ingested, and absorbed by the Portal Vein, and conveyed to the liver, in which they directly excite neuralgia.

I have had occasion for four or five years to observe some examples of hepatic colic arising from this sort of cause. I shall relate them successively, enumerating only those details which are most important.

1. M. X., affected for several years with hypertrophy of the liver, with projection of this organ below the false ribs of the right side, was subject at the same time to a painful sensation of weight, load, and swelling in the right hypochondriac region; which sensation followed regularly after taking food, and lasted three or four hours. In the course of February 1846, this symptom was progressively aggravated. The bowels were constipated.

On the 22d of the same month, M. X. took among other articles to breakfast, baked apples, something acid; and some minutes after eating the fruit, he felt all at once a sensation of burning heat, which spread from the epigastrium into the whole right hypochondriac region. This was immediately followed by painful tension in the part of the liver which extended beyond

* M. Andral has for a long time been opposed to this as an exclusive cause of hepatic colic. "We observe sometimes," he says, "in the hepatic region, pains extremely acute, which cannot be explained, after death, by any lesions in the liver, or in its excretory ducts. This forms the case of certain hepatic colics."—*Clinique Medicale*, Tome. iv., p 194. Paris, 1831, 8vo.

the ribs. This tension lasted only about twenty minutes, and it was then succeeded by a sense of constraint and load which was, so to speak, continual for the space of one month.

In the evening, M. X. began dinner with sufficient appetite. After soup, he drank two glasses of Madeira; but four or five minutes afterwards, he experienced anew, as in the morning, a burning sensation which extended from the epigastrium into the whole right hypochondriac region, where it was followed by considerable painful tension, with difficulty in breathing. M. X. was obliged to leave the table. He went out and endeavoured to walk strenuously, in order to dissipate the great pain which he suffered in the right hypochondriac region. At the end of one hour and a half it became more endurable. M. X. returned to the house, went to bed at ten o'clock, and prepared to take a bottle of Pullna water, in order to obviate constipation, which he regarded as the cause of the symptoms. But some minutes after having swallowed the first glass, the liver became once more the seat of considerable painful tension, more intense than ever. Respiration was performed with anxiety, interruptions and sighs. The patient changed his position every moment. He brought up by vomiting, first the glass of Pullna water, and afterwards yellow bile. The pain was so violent in the right hypochondriac region, that the slightest pressure could not be borne, not merely upon the projecting portion of the liver, but also upon the costal wall by which the organ is covered. Percussion showed that the whole liver was increased in volume. There was neither thirst nor frequency of pulse.

These symptoms continued at much the same rate all night, the whole of the day of the 23d, and diminished only on the 24th. On the evening of the 23d, jaundice made its appearance, and increased to the 25th. The patient did nothing but drink solution of barley water syrup, and apply cataplasms over the hypochondriac region.

On the 25th, the patient was able to rise. Some pain and swelling of the right hypochondriac region remained, but greatly less than formerly. The amendment proceeded daily increasing; the jaundice also disappeared; but the patient remarked that whenever he took undiluted wine or acid condiments, he felt in the site of the liver, an aggravation of dull pain, which lasted during the whole time of digestion. Upon one occasion among others, after having eaten radishes which had a strong taste, he felt a violent pain in the right hypochondriac region with swelling and breathlessness, a genuine attack of colic, in short, which lasted three hours, and suddenly ceased in the course of some seconds.

Two years afterwards, other attacks of colic ensued under the

manifest influence of the *ingesta*. In May 1848, the liver once more became the seat of constriction and weight at each period of digestion. Notwithstanding this warning, M. X. took one day at dinner sallad dressed with vinegar. He rose from table with a sensation of load considerably greater than the previous days. This sensation gradually increased, then all at once in the night it was converted into colic, properly so called, with pain, swelling, breathlessness, moaning, &c., which colic lasted till morning, and all at once ceased at 7 o'clock. M. X. rose as usual, attended to his ordinary occupations, having in the right hypochondriac region only a trifling feeling of pain left. In the afternoon he took a glass of natural Vichy Water, (the Hospital spring), according to advice which was given him. But some minutes only after taking this water, the pain, swelling, and other symptoms, returned quite as intensely as during the night, lasted three hours, and suddenly ceased in some seconds. After the second occasion, the urine was strongly coloured yellow and turbid. Next morning appeared slight jaundice, which receded in the evening.

During the same year other attacks took place, always under the influence of *ingesta*. M. X. resolved at length to go to Vichy. The waters were well endured, and greatly diminished this painful susceptibility of the liver. It is requisite to add, that, after these numerous attacks, very different in intensity, no gall-stone was at any time voided.

2. In the month of April 1846, there came under my care at the Hotel-Dieu-Annexe,* where I then was, a woman, aged twenty-six years, who informed me that she had been ill with *hepatitis*. This medical term she had learned in another hospital in Paris, where she had remained several weeks before entering that of the Hotel-Dieu-Annexe.

In point of fact, this woman herself made the attendants observe the liver, which protruded from four to five centimetres beyond the right false ribs, from the epigastrium to the iliac region. The hepatic protuberance was so much the more easily felt, that the abdominal muscles were relaxed and attenuated in consequence of previous pregnancies. The liver was moderately painful on pressure, not only on the prominent part, but also in its whole extent. When it was attempted to circumscribe this painful sensation by means of percussion, the patient made a gesture indicative of pain, as soon as the examiner proceeded from the sonorous portions to the part where the sound was dull.

This patient stated that this pain became sometimes intoler-

* An additional part of the Hotel-Dieu for the accommodation of patients.

with swelling and breathlessness. These symptoms lasted three hours.

On the 6th, the appetite was increasing. Food without wine was allowed.

On the 10th, two meals were allowed, and wine diluted with much water. This was well borne.

The patient proceeded to convalescence. He left the hospital on the 20th, no longer complaining of pain in the liver, even on pressure or percussion. This organ, nevertheless, projected sensibly beyond the ribs.

4. In the course of April 1846 I was called in consultation in the street Beaubourg, to a young workman of about twenty-four years, of good constitution, who for the space of two days had suffered from violent colic pains, and who since the previous evening had a well-marked attack of jaundice. He was then suffering the most acute pains, in great agitation, and with the respiration interrupted.

His wife, who seemed intelligent, informed me that her husband, though in habitual good health, complained often, after taking food, of dull pain in the right side; which she ascribed to the excessive taste of her husband for mustard. For some time he had experienced adverse circumstances; and with these, the dull pains of the right side had increased. At last, two days ago, after dinner, at which he had taken his usual dose of mustard, he felt no longer a dull pain, but a violent colic, which lasted since that time. At first, he brought up by vomiting the food ingested, and then yellow bile.

Upon examining the patient, I found a firm resistance under the right false ribs; the slightest pressure was extremely painful. Percussion denoted increased size of the liver; as soon as we passed from the sonorous parts to the seat of the hepatic dullness, the patient complained of acute pain. The point to which the patient referred the most violent spontaneous pain was the posterior part of the three last false ribs on the right side; the heat of the skin was natural; there was scarcely any thirst; the pulse was 76.

The patient was placed in a long-continued warm bath, which was repeated next day. In the interval between the two baths leeches were applied to the anus. Ptisan of sweetened gum was given.

I learned from the ordinary medical attendant, that after the application of the leeches the pain was abated. Three days afterwards it was so slight, that the patient could quit his bed. At the same time the jaundice disappeared, and gradually the patient took food as usual.

5. On the 30th of May 1848, I observed at the hospital Saint Antoine, the case of a female sufficiently similar to the one

8 A person, aged sixty-four years, an old soldier, of strong constitution, experienced, from time to time since 1810, pains more or less violent in the right hypochondriac region, without its being possible to say exactly whether they were or were not those of hepatic colic; this, nevertheless, was probable, if attention is given to what took place subsequently. This man was strongly attached to the use of red wine; and rather often it happened to him to drink four or five *litres* of this liquor in the course of one day, without eating, yet without ever presenting notable symptoms of intoxication.

On the 27th of June 1850, he spent the day in libations with a friend, and drank his habitual allowance of five *litres* of wine. He went to bed at ten o'clock, after having attempted to eat some soup; but as he was not hungry, he took no more than two spoonfuls.

After midnight, about two o'clock, he was awakened by a violent pain in the right side of the chest, which obliged him to moan, and threw him into extreme agitation. At ten o'clock in the morning M. Leclaire, his physician, found that the pain occupied the whole of the right hypochondriac region and the epigastric. The patient could not bear in these parts the slightest pressure. The pulse was natural. There were neither stools nor vomiting. The urine voided by the patient was of the acajou colour;* the evening before it was natural. The conjunctiva was slightly jaundiced. M. Leclaire, persuaded that it was gall-stone colic, prescribed the potion of Durande.† This the patient took at two o'clock; but some minutes after the first spoonfuls the hepatic pain, which had been slightly abated after mid-day, underwent a remarkable aggravation. The patient screamed, was agitated, could no longer take the potion, declared he was poisoned. The pain lasted in this form till next day, the 29th, when it suddenly ceased at seven o'clock. The patient was able to rise; he had a little appetite and took some soup. The jaundice, which began on the 28th, was increased.

On the 30th, the patient was purged by means of Seidlitz water. He discharged bilious stools but no concretion.

On the 2d of July the patient thought he might eat a cutlet and drink wine diluted with water. But immediately afterwards he was attacked by intense hepatic colic, which lasted for three hours and suddenly ceased.

On the 3d, 4th, 5th, and 6th, the patient took only soup and *potage*, and abstained entirely from red wine. He went on improving, the jaundice declined daily, and he had only a slight tinge of yellow.

* The *Acajou* is the *Anacardium occidentale*, or Casheir Nut Tree. This seems to refer to the colour of the receptacle, which is crimson.

† A mixture of turpentine and sulphuric ether.

On the 7th, the patient, finding himself well, eat without hesitation a Bret-fish, and drank a glass of undiluted wine. This indulgence was followed by the return of the hepatic colic in its original violence, and lasting longer than ever, for three days. The jaundice also reappeared in all its intensity. M. Leclaire ordered leeches to be applied over the hypochondriac region.

On the 10th, the pain was gone; the jaundice was deep. Soup was allowed.

On the 13th, the jaundice had almost disappeared. Castor oil procured bilious stools but no gall-stone. Soup; then more substantial food. No wine allowed.

The patient continued to improve, and punctually followed the regimen.

On the 15th of August he broke the regimen ordered. He had gone at noon to the barrier with a friend and drank one *litre* of undiluted wine. Immediately afterwards hepatic colic came on. The patient, alarmed, ran into the fields with the view of dissipating the pain, but in this he did not succeed. At two o'clock he came to his physician, who ordered him to bed, and to take a soothing draught. The colic ceased at six o'clock the same day, and was followed by slight jaundice which declined next day. Since this last affair the patient has renounced the use of undiluted wine, and he feels in the right hypochondriac region only trifling and fleeting pains. The liver makes a slight projection in the epigastrium.

The facts now adduced appear to me to demonstrate in a positive manner the actual existence of hepatic colics, produced by the conveyance into the liver of certain articles of food and drink which have acted as irritants to this organ. Though I am unable to explain these results according to the present principles of pathology, they have formed the basis of all the ideas contained in this memoir.

This disorder is not common. It is still less so in hospitals than in civil practice. Nevertheless, if we see not frequently attacks of hepatic colic, properly so called, that is to say, attacks of violent pain, with anguish, breathlessness, and the other symptoms, we observe, frequently enough, persons who, after having taken into the stomach irritating substances, feel a sensation of uneasiness, oppression, and heat in the hypochondriac region.

We must place in the first rank of substances which possess the disagreeable faculty of exciting hepatalgic attacks in individuals predisposed, at first alcoholic drinks taken even in moderate quantity; then green acid fruits; strong acrid condiments, such as mustard, pepper, salt, fruits preserved in vinegar, and similar

We are led to distinguish in hepatic colic as in asthma and in gout, the *attack*, which may last for eight days, fifteen days, one month, or which exists, in short, during the whole time that the liver may be affected by pain; and then the *accession* or *paroxysm*, which is nothing else but the colic properly so called, which lasts from one to two days, and which is repeated more or less frequently so long as the attack is continued, that is to say, so long as the liver is irritable.

It will also be necessary to distinguish the colic from different causes, according to certain characters peculiar to each of them. Thus we may consider as calculous, the colic which is not accompanied by bilious vomitings, which is terminated by the expulsion of a gall-stone, and which is little liable to return by relapse. The rheumatic colic is that which appears under the influence of chilling or a rheumatic metastasis, and which declines on the re-establishment of warmth and transpiration. Lastly, the colic arising from ingested articles will be considered as such, when it is manifested immediately after a meal, when the initial pain proceeds from the epigastrium into the right hypochondrium, when the duration of the colic hardly exceeds that of digestion, and when another attack comes on after the ingestion of any irritating substance. It is well understood, that in these two last species of hepatic colic, bilious vomitings and frequent relapses may take place.

I have every reason to think that hepatic colic owing to *ingesta* is the one which there has been the most frequent occasion to observe. The proof of this I find in respectable authors who have treated of the calculous colic, and who, preoccupied by the established doctrine of the effects of gall-stone, refer without hesitation to the alleged presence of gall-stone, the circumstances symptomatic of the colic determined by the *ingesta*. Of this I shall now furnish proof.

I shall adduce first the authority of the nosologist Sauvages. According to this author the principal characters of *hepatalgia calculosa* are the following. “*Hepatalgia calculosa cognoscitur 1mo, ex atroci sæpius dolore circa locum ubi canalis choledochus inseritur duodeno; 2do, ex eo quod dolor ille ad costas spurias et epigastrium extendatur; 3tio, quod tribus sæpius a pastu horis, recrudescat; 4to, quod auriginem comitetur aut sequatur.*” *

One of the characters of *colica calculosa*, then, assigned by Sauvages is, that this colic evinces its presence most frequently three hours after the ingestion of food. In what manner is this fact to be understood as to calculous obstruction? It must be understood that the gall-stone is found in the *ductus choledochus*

* *Nosologia Methodica*, Tome ii., p. 107.

"I attended professionally in 1838 General — for symptoms of violent hepatic colic, the accessions of which were frequently renewed. One of them continued for ten hours, and threw the patient into a state of despair; when Dr Louis, who was called in consultation, recommended the administration of a vapour bath. Scarcely had the patient been in the bath for five minutes, when he announced with joy that the pain had suddenly passed away."

It is possible, viewing this case strictly, that the disease was in this case dependent on calculous colic, and that the bath had effected the sudden expulsion of a gall-stone. But it appears to me more probable that the *hepatalgia* was of a rheumatic character, and that the induction of a copious perspiration was sufficient to cause the speedy dispersion of this visceral neuralgia.

The same I shall say of those important facts, by means of which M. Bricheteau has been led to recommend the external application of ice in the treatment of hepatic colic. In the Memoir, in which these cases are recorded* we perceive that applications of this kind have dissipated in the course of four or five minutes attacks of the most violent colic. A question here arises, whether these pains depended on the circumstance of the biliary passages being obstructed by a gall-stone, which may have been displaced by the action of the ice; or is it not more probable that the ice may have produced relief by the sedative power which it exerts in the treatment of neuralgic disorders? For upon this point we must not lose sight of the fact, that, besides hepatalgic attacks from *ingesta*, which are frequent, and hepatalgic attacks from gall-stones, which are rare, it is requisite to admit the existence of rheumatic hepatalgia, perhaps even gouty hepatalgia, hysterical hepatalgia, &c., each of which requires a particular method of treatment; and whatever be the cause or the nature of the colic, the *ingesta* of agents slightly irritant, fail not to excite at the very moment an attack, when the disease is in its early stage, and that the liver is then in an extremely irritable condition.

HEPATITIS.

We shall endeavour to show that there are attacks of *Hepatitis* produced by the direct and local influence of ingested articles upon the liver. This article is in a sense the consequence of the preceding one; for if certain substances are capable of producing a fleeting and temporary disorder of the liver, distinguished by violent pain with swelling and determination of blood into the organ, there is no great distance be-

* Clinique de l'Hopital Necker, p. 334. Paris, 1835.

tween this and the production of a phlegmasial congestion, fixed and continuous, that is to say, Hepatitis.

This disorder is seen in its greatest intensity only in hot climates, and particularly in India and the Antilles, where many persons are attacked by it. It is then only by means of documents furnished by physicians who have practised in these different countries that we can establish the pathogenetic point under consideration.

All physicians are agreed in admitting the hurtful influence of alcoholic drinks and irritant aliments upon the formation of hepatitis. Annesley allows that among all the agents which in Bengal tend to induce inflammation of the liver, few possess so much energy as the habit of alcoholic drinks.* Another author, who has also practised in Bengal, Dr Twining, informs us, that the immoderate use of wine, alcoholic liquors, and stimulant articles of food, expose the greatest part of Europeans who inhabit India to suppurative inflammations of the liver.† According to him we must see in this circumstance and in the atmospheric vicissitudes so sudden in India, the twofold cause of the attacks of hepatic inflammation which are endemial in that climate.‡

The same author furnishes valuable information upon the period of the appearance of *hepatitis*, the commencement of which he fixes immediately after taking food.§ This peculiarity connects hepatitis beginning in this manner with the attacks of hepatic colic produced by certain *ingesta*, and shows, in the collateral appearance of the two diseases of the liver an exciting influence depending on the nature and perhaps the amount of the articles introduced into the stomach.||

* "Among the various influences which more directly occasion the supervention of inflammatory action in the liver there are few more energetic than the immediate addiction to the use of spirituous liquors."—Annesley, *Diseases of India*, vol. i., p. 488. London, 1828.

† Habitual plethora and superabundance of wine and spirituous liquors, as well as of stimulant food beyond the real wants of the constitution, doubtless keep the greater number of Europeans in India in an almost perpetual state of proclivity to inflammatory and suppurative disease of the liver."—*Diseases of Bengal*, p. 247. Calcutta, 1835.

‡ Ibid.

§ "The attack sometimes commences suddenly after eating."—Ibid. p. 244.

|| It may be admitted that if irritating articles, and among others alcohol, produce so easily in hot climates attacks of inflammation of the liver, it is because in these climates the porto-splenic blood possesses little assimilating power, and allows the different agents to penetrate to the liver, without having previously induced in them modification or destruction. It is permitted further to think that this defect of assimilating power partly depends on this circumstance, that in consequence of heat, and the rarefaction of the air, the porto-splenic blood contains an insufficient quantity of oxygen. The contrary of this is seen in cold countries, in which the blood, which ought there to be very highly oxygenated, possesses a great power of assimilation. Persons worthy of credit, who have

A natural question arises, in what manner do ingesta act in producing *hepatitis*?

We know that Broussais imagined that they caused inflammation first in the duodenum, then in the common bile-duct and in the hepatic duct, and, lastly, in the liver. Upon this idea he had founded his doctrine of *hepato-duodenalis*, which forms one of the hypothetical diseases of the doctrine called physiological. Physiology, nevertheless, hardly permits an hypothesis like this, for it informs us that a great many irritating substances, and especially alcoholic drinks, to which Broussais ascribed particularly great influence in the production of hepatitis, are absorbed in the stomach, and consequently do not proceed to the duodenum.

Many authors, who have not been able to admit this opinion of Broussais, have said that the exciting substances irritated the stomach, and sympathetically caused inflammation in the liver, in consequence of the union which connects together the different parts of the digestive apparatus.

This explanation might strictly be regarded as of some value, at a period when it was thought that absorption was affected by the lymphatic vessels, and the matters absorbed did not pass through the liver. But considering that the conveyance into the liver of the matters absorbed by the gastro-intestinal veins is regarded as an irrefragable physiological fact, we are obliged, it appears to me, to admit, that if irritating substances cause inflammation in the liver, they produce this effect directly and by contact; and it is not necessary from this result that they have previously caused irritation in the intestinal tube. For as we sometimes see the large intestine inflamed by means of substances which have come to it, yet without producing any alteration in the superior parts of the digestive tube, in like manner the liver may undergo an isolated inflammation, from the contact of matters which have traversed the stomach, the intestine, and the

travelled in Russia in a cold of 25°, have informed me that they have drank large glasses of distilled spirits without experiencing from them the slightest uneasiness.

(The argument and reasoning now adduced seem to be open to fallacy. No conclusion can be drawn from statements made as to no present inconvenience being felt from drinking quantities of distilled spirits in cold countries and cold seasons. Many observations show that these articles do not maintain the heat as is commonly believed, but rather diminish the power of maintaining the heat. But this is not the question. The point requiring determination is, whether the use of spirituous liquors does not induce in cold countries many diseases, especially those of the liver. Now of this there can be no doubt. All testimony and experience go to prove that the use of these articles is the principal cause of hypertrophy, *kirrrosis*, and similar affections of the liver. Indeed *kirrrosis* appears to be the malady of the liver in cold climates, as *hepatitis* is in warm and tropical countries. In England, Scotland, Holland, and many parts of Germany, it is a common disease.)

so as not to return. But in such circumstances it acted as an antineuralgic upon the liver, and it exerted for the cure of hepatalgic attacks a specific action quite different from that, which has been gratuitously ascribed to it as a solvent of gall-stones.

Admitting that certain evacuants penetrate by means of the Portal Vein to the liver, it would be requisite to explain in what manner these agents clear out at the same time the intestinal canal; but nothing prevents us from adopting this opinion, that the agents before mentioned, having once arrived at the liver, are in it subjected to an elective action, by virtue of which the liver eliminates them from the mass of the blood, and causes them to flow with the bile into the biliary vessels, by which they again enter into the digestive tube. It is very possible that the evacuants, which are supposed to excite an abundant secretion of bile, and which, from this effect, have been justly denominated CHOLAGOGUES by Galen, act in the manner now specified. This question might be easily determined by means of experiments and vivisections.

Not only the alimentary articles, however, even of the most exciting properties; not only medicines arrive at the liver by the channel of the Portal Vein;—the most soluble poisons follow the same route before penetrating into the entire organism and infecting the mass of blood. In proof of this inference, I have only to recal to the reader's remembrance the experiments of M. Orfila upon the subject of arsenical poisoning;—experiments which have in all instances shown the presence of a large proportion of poisonous substances in the liver. The reason of this superabundance it is easy to perceive. The liver being placed partly as a barrier, partly as a sort of filter between the system of the Portal Vein and that of the *Vena Cava*, it is quite natural that the poisons which come to it from the digestive tube, by the current of the Portal Vein, and which must of necessity traverse the organ, should accumulate in its interior in greater quantity than in other organs.

Such are the different pathological facts, which show that matters ingested into the digestive tube are conveyed to the liver by means of the Portal Vein. These facts form as it were the corollary of the numerous experiments, which have been performed in recent times upon the absorbing faculty of the Portal Vein; and the concordance in this respect between physiology and pathology is perfect.

Notwithstanding this, these pathological facts have never been noted, even at the period at which the conveyance of aliments into the liver was admitted as one of the most un-

We behold at first the engorged spleen becoming the seat of a suddenly advancing pain, when the individual attempts any considerable exercise. This depends on excessive engorgement of the spleen, in consequence of the fulness or large quantity of blood which, we know, is found in the Portal system during locomotion and muscular contractions.

We know also that this fulness is carried to its greatest height when at the same time locomotion and introduction of alimentary articles into the *vena portae* concur. This explains the reason why the patient was obliged, on account of the painful distension of the spleen, to remain in a state nearly motionless during the whole time of digestion, unless he had taken only a small quantity of food. Clinical observation could not afford a stronger or more pointed confirmation of the experiments performed by Mr Dobson on dogs deprived of the spleen, which, it may be remembered, all presented symptoms of considerable plethora, only after taking copious meals.

No notice has been taken in modern authors of this hurtful influence of violent locomotion upon persons affected by splenic disease, especially after taking food; but it is perfectly indicated in the monographical treatises of the old physicians. For instance, Flammerding, among the hygienic precautions which he recommends to individuals labouring under tumours of the spleen, justly enforces the following direction; “*exerceat se et ambulet, moderate tamen, ante cibos.*”* Drelincourt, the son, supporting his opinion upon the strength of respectable authorities, gives the same recommendation to the same class of patients; but this he does in terms very energetic, in so far as he represents immoderate exercise after meals as a thing to be eschewed worse than dog and serpent. “*Sed ambulationem vehementem cum pejus et angue lienosus declinet statim a pastu, uti prudenter atque perite moneat Haly Abbas, Alexander Benedictus, atque Lucas Tozzus.*”† Lastly, the same precaution is strongly specified in the Dissertation already mentioned, as bearing the singular title *De Porcello Cassoviensi*. Adelphi, the author of this performance, states that the swelled spleen is the seat of a pain which is augmented when the patient attempts muscular exercise, immediately after having taken food. “*Is dolor augetur maxime protinus a cibo exercentibus corpus, altiore ascenditibus locum, currentibus.*” And he naturally recommends that the patient should repose after taking any meal.

It is requisite, meanwhile, to enforce this wise and rational recommendation, not only in attempting the treatment of these instances in which the spleen is very much enlarged, but parti-

* De Tumoribus Lienis Leydæ 1671, Theses 28.

† Dr. Drelincourtii Opuscula. Hagæ Comitum. De Lienosis, p. 782.

some writers have ascribed dropsy to deficient power in the lymphatics, this was certainly at no time regarded as the sole cause of dropsical diseases. There is on the other hand sufficient evidence in the writings of anatomists, pathologists, and physicians, to show, that dropsy was believed to arise not uniformly from any single cause, but from several; and that even the obstructed state of the veins was not entirely overlooked as a cause of dropsical effusion. The time at which the deficient absorbent power of the lymphatics was admitted among many, was probably about the middle of the eighteenth century. But the verisimilitude of this doctrine was greatly weakened by the frequent difficulty and occasional impossibility that was experienced, in exciting by medical treatment the action of the lymphatic vessels to cure dropsy. In no instance was this more strongly evinced than in the theories of the cause of water in the brain; and the frequent insanability of that disorder, must have been a strong argument against the supposed influence of the lymphatics in producing dropsy.

It would lead to too great digression to give of this doctrine of dropsy such a history as the subject deserves. But some of the notions prevalent on the nature of this disease during the eighteenth century may be understood from the following facts.

In the first place, in so well known a book as the *First Lines* of William Cullen, the doctrine that obstruction to the circulation in the veins is a frequent cause of dropsy, both general and local, is distinctly stated in paragraphs 1648, 1649, 1650, and 1651 of that work.

In the last mentioned paragraph his words are the following.

“Not only those causes interrupting the motion of the venous blood more generally, but further the interruption of it in particular veins, may likewise have the effect of increasing exhalation and producing dropsy. The most remarkable instance of this is, when considerable obstructions of the liver prevent the blood from flowing into it from the *Vena Portarum*, and its numerous branches; and hence these obstructions are a frequent cause of dropsy.”

The *First Lines* of Cullen were published in 1777; and the last edition corrected by the author was in 1784. Consequently, this doctrine must have been known at that time.

2. In the second part of his *Experimental Inquiries*, published in 1774, William Hewson denies that either “increased secretion, or impeded absorption, or rupture of a lymphatic vessel, gives rise to such morbid collections of water as are found in dropsies;” for the reason that the fluid let out does not resemble that contained within the cavities of the serous membranes in

the cavities of the cellular membrane, whilst the lymphatic vessels, having lost their tone, do not take up these fluids. Cases of this kind are frequently met with in young people from temporary debility, who have undergone some tedious and dangerous disease, as after fever. From this they frequently recover; but it is a very bad symptom in old age, accompanied with asthma, as it is most probable, that the visible dropsy of the legs is then accompanied with an invisible one, or water in the chest. Women frequently bear this swelling of the legs longer than men, even for years, without any danger. Some men have sustained it for twelve years, without the least decrease of their general health. I have seen the integuments of the ankles hanging over the shoes for months, and yet the patient has perfectly recovered.

“The second species of dropsy is very common, and is that which arises in consequence of previous inflammation of a cavity; and may take place in any habit of body. If an inflammation arise in a cavity, it may terminate in a number of different ways: one of these ways is by an increased secretion of the fluid of surfaces.

“A man receives a blow on the testicle; inflammation takes place, and the consequence is frequently a hydrocele, or dropsy of the tunica vaginalis. A child's brain inflames, and this inflammation ends at last in hydrocephalus, or collection of water in the brain. Pleurisy frequently terminates in hydrothorax, or collection of water in the chest. I have often taken away forty or sixty pints of water, which had accumulated in the cavity of the abdomen, in the few days the peritoneal inflammation had lasted, during the usual species of the child-bed fever. This is to be considered as the substituting a less dangerous disease for another. Peritoneal inflammation kills often in three days, but ascites may last twenty years.

“When the arteries of the part have once got a habit of increasing their secretions, they commonly go on for a long time. The lymphatics may, in some cases, absorb their usual quantity of the fluid of surfaces; but, as the fluid secreted by the arteries often far exceeds the quantity which they absorb, the dropsy is still kept up; or, the lymphatics may be so altered by the inflammation, as not to absorb a sufficient quantity.

“The third species of dropsy is that which arises from some obstruction to the return of the venous blood to the heart. This may happen from the blood of the vena portarum being obstructed in its course to the heart by a skirrhus liver; ascites, or dropsy of the abdomen, may be the consequence. It may happen from inflammatory adhesions of the substance of the

autem oedema nascitur, si sanguis non per. venas refluxere potest.”*

If, therefore, the fact again made known by Bouillaud were a discovery, it is so by neglecting and overlooking doctrines which were well established,—a source not unfrequent of discoveries.

Note (B), p. 294.

It is not perfectly correct to say, that in the instance of suppuration of the liver, associated with dysentery or ulceration of the intestines, the purulent matter directly excites inflammation. It seems more accordant with the fact to say that the purulent matter is simply conveyed and transported by the veins of the intestines to the *Vena Portae*, and in its ramified branches is there deposited in the substance of the liver. Unquestionably it may produce inflammation. But it is manifest that this is not necessary; simple transport being all that is requisite. The same, it is known, takes place in the *pleura* and in the lungs, from suppuration at the extremities or in the course of veins; and in wounds and injuries of the liver these secondary collections are often seen.

An exception has been taken to this doctrine, that abscess and purulent deposits in the liver do not take place in all cases of dysentery. The objection is perfectly well founded. It is not in dysentery, properly so called, that is, in its early and inflammatory stage, that purulent deposits in the liver are observed. It is only when the disease has proceeded to ulceration, and when ulceration has taken place in the intestinal tissues from other causes, that purulent deposits in the liver take place, or may take place. Let it *first* be remembered, that, to the formation of these purulent deposits a suppurative or an ulcerating surface is requisite; that is, a surface secreting purulent matter, which may be taken up and transported by the veins. *Secondly*, let it be also remembered, that the purulent matter may be secreted, yet may not be absorbed by the veins. Unless, indeed, it is both secreted by the surface and absorbed by the veins opening on that surface, no purulent deposit can be formed.

In the *third* place, let it be remembered, that, in the circumstances stipulated, there is not one but many ulcerating and suppurating surfaces; and it will not be difficult to understand why, in certain cases of dysentery, that is, in the early stage and the inflammatory condition, purulent deposits do not take place in the liver, and why in the ulcerative and suppurating stage, and

* Joannis Zachariæ Plattner, D. et Prof. Med. Lips. Institutiones Chirurgiæ Rationalis, tum Medicæ tum Manualis in usus Discentum. Editio altera. Lipsiæ. 1758. 12mo. Editio Novissimis. Lipsiæ, 1783.

If, therefore, this formation of low sugar begins in the stomach, surely it follows that the liver cannot be the sole and exclusive sugar-making organ in the animal body. Either it must be merely the recipient of sugar for some particular purpose, or it can perform with the stomach and duodenum not more than only a concurrent and auxiliary part in the formation of that substance. In either case the fault cannot be ascribed solely to the liver, that sugar is found in the blood and in the urine.

Let it be remembered, also, that it was clearly proved several years ago by Mr Macgregor of Glasgow, that in diabetes, sugar was found in the stomach, and was formed in that organ.

As to the sugar being destroyed in the lungs, the expression is altogether inapplicable, and communicates no just idea of what takes place in these organs. No doubt oxygen is received, and carbonic acid is discharged by these organs; and it may be admitted to be possible, or even probable, that the sugar is decomposed; that is, that its carbonaceous matter is united with some of the oxygen to form the carbonic acid. But we further know that the system cannot exist without carbonaceous matter; that carbon is found in the muscles, in the bones, in the membranes, in most of the viscera; in short, in all the animal substances.

It seems, therefore, most natural to infer that the saccharine matter which is found in the liver, is merely in that transition state in which it is most easily applied to the nutritious necessities of the system; that its existence as sugar has reference to the primary acts of digestion rather than to any destructive action supposed to take place in the lungs; that as vegetable matters, furnishing sugar, constitute the principal articles of nutrition for animals, so sugar is formed and found in the intermediate stage, though not in the form of sugar in the final stage of nutrition; and that in the final assimilative process in the lungs, the alleged saccharine matter merely undergoes that degree of transformation, which allows the carbonaceous matter to be applied to the repair, the formation, the growth, and the permanence of the animal tissues.

Let it be remembered in these remarks, that pure crystalline sugar does not, and cannot, for a perpetuity, sustain and nourish the human body; that, in the crystalline form, it is positively and unequivocally hurtful and innutritive; that it is always presented to the stomach in the form either of amylaceous matter or general vegetable matter; and that in all animals hitherto examined, the saccharification of these articles begins and proceeds in the stomach. The natural inference from these facts seems to be, that the stomach, *duodenum*, and

ART. IV.—*On the Structure of the Membrana Tympani in the Human Ear.* By JOSEPH TOYNBEE, F.R.S., Fellow of the Royal College of Surgeons of England, Aural Surgeon to St Mary's Hospital, and Consulting Surgeon to the St George's and St James's General Dispensary. (From the Philosophical Transactions for 1851, Part I.)

IN conducting researches into the pathological condition of the organ of hearing, I have been impressed with the necessity of ascertaining, with as much precision as possible, the intimate structure of the various tissues entering into its composition. The results to which investigations undertaken with this latter object in view have led me, in reference to the structure of the *membrana tympani*, it is the purpose of the present paper to detail. It will be observed that the conclusions at which I have arrived, differ materially in various important particulars from those of previous investigators.

Examined from the exterior, inwards the *membrana tympani* consists of the following layers:—

1. The epidermis.
2. The proper fibrous layer, composed of—
 - a. The lamina of radiating fibres.
 - b. The lamina of circular fibres.
3. The mucous membrane.

1. The *epidermis* is a thin layer covering the outer surface of the radiating fibrous lamina; it is continuous with the epidermis of the external meatus, and when subjected to the process of maceration it can be removed in the form of a small blind pouch, which presents, as it were, a cast of the meatus and of the external surface of the *membrana tympani*. When floating in water the pouch assumes the form it had when in contact with other tissues, and its internal extremity is convex, corresponding with the external concavity of the *membrana tympani*. The layer of epidermis forming the outermost coat of the *membrana tympani* is thin, and in the living subject so transparent that the radiating fibrous layer is distinctly seen through its substance: its outer surface is very smooth and capable of reflecting light: this layer presents no appearance of an orifice.

2. The *proper fibrous layer* of the *membrana tympani* can be easily separated into two laminæ, which, on account of the direction of their component fibres, may be called the radiating fibrous and the circular fibrous laminæ. Previous to entering upon an examination of these structures, it is desirable to cite the opinions which eminent anatomists have entertained on the subject.

In the Croonian Lecture, published in the ninetieth volume of the Philosophical Transactions, Sir Everard Home advanced the opinion that the *membrana tympani* in the human subject was

1. Above the lower jaw, closed at an early period, and bounding as it were below the field of the embryogenic process.

2. The advancement towards the buccal centre of the three appendages of the cerebral cellules; one median (naso-incisive) proceeding from above; two lateral (the superior maxillary bones).

3. The nose forming itself in the unmatched appendage, or that on the median line.

4. The mouth gradually circumscribing itself by the meeting of the three appendages.

5. Lastly, a formation more deeply seated, and synergically giving rise to the palatine arch, as if a dependence of the lateral appendages, to the *septum narium*, a manifest offshoot from the naso-incisive appendage.

There is no necessity for lengthened commentaries in order to show how the explanation of the true nature of hare-lip, and of all its varieties naturally flows from this succinct description. To take a striking example, any one may perceive in the cases altogether extreme, in that deformity which is popularly named Wolf-Throat, the persistence in its entire extent of one of transition states of the embryo.* In this, as on the thirtieth day of foetal existence, the observer beholds the nasal and buccal cavities forming only one large common funnel, the separation of which is scantily indicated by the *palatine processes* detached from the internal surface of the superior maxillary bones. Then internally between the separated halves of the jaws and of the lips, he observes the isolated naso-incisive eminence, (soft parts and hard parts), strongly projected forwards, and supporting behind the *septum* of the nose, which remains suspended and free in all the other directions. He may be struck, in this instance, as in the foetus, by this nasal dependence of the upper lip and upper jaw, by this slow and distant complement, which advances from above to join itself to the jaw.

What has now been said is applicable to the extreme cases.

Imagine next all the possible degrees of transition to the most simple form of hare-lip; for under this form, which is traced in the face of the embryo, we must not attempt to consider the skeleton apart from the soft parts. This mode of separating the bones from the soft parts in these reasonings has probably been productive of errors, particularly in reference to the *osteo nervous axis*. In this member, thoracic or abdominal, which at the fortieth day, for instance, may be perceived pointing under the form of a shoot or sprout, all the elements, cutaneous, muscular, bony, vascular, were virtually included; and the proof is, that, if its evolution is stopped, all these different elements are observed in it at birth. In like manner, the incisive shoot or sprout, consisting at first of one almost single *blastema*—of cellules nearly similar

* See for instance the series of anatomical preparations in the Collection of the Dupuytren Museum.

of Guggenbühl, hold the first rank.* If the authors have added few facts to those which previous anatomical inspections of bodies had communicated, they have nevertheless given to the anatomical study of the distemper a new direction. In order to show and enable readers to form an estimate of this scientific movement, which is in a great degree owing to the initiative now mentioned, it is sufficient to advert to the inquiries and the writings of Stahl.

Charles Frederick Stahl has published two memoirs on the subject of Cretinism. The first is inserted in the *NOVA ACTA ACADEMIAE CURIOSORUM NATURÆ* for 1845.† The second was published in 1848, and procured for the author one of the prizes awarded by the Institute (Academy of Sciences) in 1850.‡ This two-fold monography has been so often quoted in the course of this discussion, opened at the Academy of Medicine, that it seems proper to give here an analytical account of it. The two essays possess further much true interest.

In proportion as our knowledge of Cretinism has been extended, as pathological observation has passed from travelling physicians to physicians resident in the Cretin districts, many analogies, striking at first sight, have disappeared before the light of a more mature examination. Unperceived differences have been discovered. The unity, something artificial of Cretinism, has not remained quite entire. In a geographical point of view, even authentic facts have been found to contradict the opinion so strongly founded, which ascribed the origin of the disease to the moist air of deep valleys. Cretins have been found in the plain country;|| they have been met with in the midst of populations varying in aspect and in constitution. The definition of Cretinism requires to be extended, so as to comprehend those more precise and more numerous observations. Recent inquirers have recognised types in situations, in which the early authors had seen only degrees. It may be asked whether it be possible to explain by a lesion at all times identical incidents so dissimilar. The incidental affections of the Cretin, the very different nervous disorders to which he is subject, ought to lead to the supposition that the concomitant alterations had not the uniformity which they were originally believed to possess.

It is in this manner that there has been established a new tendency in inquiry, of which Stahl is evidently the representative

* *Neue Untersuchungen über den Cretinismus*, in 8vo, Zwey bande Erlangen, 1844.

† *Beitrag zur Pathologie des Idiotismus endemicus in den Bezirken Salzheim und Gerolzhofen in Unterfranken.*

‡ *Neue Beiträge zur Physiognomik und Pathologischen Anatomie der Idiotia Endemica* in 4to. pp. vi., und 77. Fig. Erlangen.

|| *Morel sur le Cretinisme de Rosieres (Meurthe)* 1851.

flattened. In the fourth subject, the median lobe was very much developed, the ventricles were full of serous fluid. The occipital *fossæ* being almost completely effaced in Cretins, the lower surface of the *cerebellum* is modified in a corresponding degree and manner; the two lobes are flattened and the large fissure is shallow. In one subject the inferior vermiform body was scarcely indicated. The valve of Vieussens was very dense. The peduncles or crura of the *cerebellum* were very delicate.

The Isthmus of the Brain.—The *tubercula quadrigemina*, were large and irregular. The *corpora geniculata* were small.

The Spinal Bulb—The Spinal Bulb always appeared smaller than natural. The Pyramidal bodies appeared to M. Niepce in some instances small, in others normal. The Olivary bodies presented the same anomalies. The basilar or cuneiform process being horizontal, the spinal bulb could not occupy the basilar groove, which does not possess the necessary degree of excavation; and the *medulla oblongata* cannot easily penetrate into the occipital *foramen*.

M. Niepce finally found that the complicated interlacement of the numerous nervous fibres, which is observed so well developed and distinctly perceptible in healthy individuals, had scarcely any existence in the Cretin annular protuberance, and that the spinal bulb was greatly smaller than it ought to be and softened.

The Medulla.—M. Niepce ascertained nothing abnormal in the *medulla*, except in its small size, and that it was immersed in a quantity of serous fluid more abundant than ordinary.

A fact which several authors had previously ascertained, he found, namely, that the *foramina lacera posteriora*, through which pass the *Nervi Vagi* and the *Nervi Glossopharyngei*, were almost obliterated and these nerves were very small. The other cranial nerves presented nothing abnormal.

Having examined carefully the auditory apparatus of one of the deaf and dumb Cretins, M. Niepce found that the diameters of the *meatuses* and of the apertures were much narrowed; that the *ossicula auditus* were spongy and larger than they generally are; the nervous pulp of the acoustic nerve was denser than it ought to be; the conduits were almost obliterated.

These anatomical results are, it may be observed, in accordance, upon almost all points, with those which have been formerly mentioned. The lesions present neither the unity which the earliest observers ascribed to them, nor the diversity which has since with some exaggeration been represented to prevail.

It was nevertheless impossible that Pathological Anatomy applied to Cretinism should not feel the re-action of the predominant opinions. The examination of the organs such as they are observed after life is extinct, the simple inspection of the solids,

are no longer adequate to account for the alterations induced by the disease. The more a distemper is generalized, the more we are disposed to search for its character elsewhere than in a local or directly appreciable disorganization. Does Cretinism not present itself in such conditions that the new means of study are applicable to it with advantage? This is the question which in late years, some physicians have proposed for the first time, and which, led away by an apparent utility, they have even attempted to resolve.

Already Stahl in his first Memoir had attempted to obtain, from the chemical analysis of the Cretin brain, information which the other modes of investigation had failed to supply. This attempt was attended with so little success, that he was not encouraged to repeat it. Dr Niepce has devoted his attention to analyses of the blood, which, however, have led him to no conclusion. *Lastly*, still more recently, Dr Erlenmeyer, Director of the Institution for the treatment of Idiots and Children Imbecile in Mind, at Bondorf, near Coblenz, has chosen, for the subject of a long Memoir, the blood, the urine, and the alvine evacuations of patients of this description.* Besides that the classification of patients is sufficiently defective, in so far as it unites forms of disease very remote from each other, the analyses have added no satisfactory information to that which had been learned by other means of observation. It will be sufficient to name only some of the propositions which form the summary of the particular researches. The specific gravity of the blood is liable to great variations; the serum is in general clear and transparent. The microscopical examination brings to sight anomalies neither in the figure of the globules, nor in the other elements; the water appears to be in more considerable proportion than in sound persons. The solid parts are in quantity variable according to circumstances; the fibrin is found in amount nearly normal; the proportion of salts is little diminished.

It may be proper to mention that the description of two Cretin skulls is given by Dr Betz in the first number of the *Journal of Rösch*, and a note from the same author appears in the second number. These articles, however, contain nothing requiring particular notice at the present time.

In the *Medical Intelligence* will be found some observations on the influence and operation of the alleged causes of Cretinism and Goitre, in reference to the *Inquiries and Researches* of Drs Grange and Ferrus.

* *Beobachtungen über den Cretinismus, Journal de Rösch, 2d band. 1851.*

it was necessary to ascertain their nature, to trace their mode of development and production, to establish the periods of their occurrence in different classes of animals, and to learn something of their chemical composition, as well also as of the fluid portion of the semen in which they move. Most of these inquiries have been well followed out by Wagner, Siebold, Muller, and more especially K  lliker, and more recently by Wagner and Leuckardt, from whose labours we have now some positive information which enables us to deduce a fair conclusion respecting their function, although a direct proof of its correctness is still to be supplied. Most observers now believe with K  lliker, that the spermatozoa (still so called) are not independent living organisms, but are merely elementary constituent parts of the male body, an opinion in which my own investigations lead me fully to coincide. This opinion, indeed, is not entirely new, as a like view was held by some observers at the beginning of the last century, when it was still questioned whether the spermatozoa are normal constituents of the semen. Dr Drake,* in his "New System of Anatomy," while acknowledging that he had seen the seminal animalcules, and combating on the one hand the theory of Leewenhoek respecting them, and on the other the view that had previously been held with regard to the ovum, doubted their separate organization, and suggested that they "may be nothing more than some large particles of mixed fluid, whose motions and different figure the microscope discovers to our eyes," &c. G. TREVIRANUS† more recently held a similar opinion, that they are not independent animals, but are analogous in their structure and properties to particles in the pollen of plants, and that their motion is of the kind discovered by ROBERT BROWN in vegetables. K  LLIKER,‡ however, first distinctly referred them to a class of known organic constituents of the living body, the vibratile cilia, a view which had previously been discussed and inclined to by MULLER.§

But however much our knowledge has become settled in regard to the nature of the spermatc bodies themselves, and their mode of development, their relation to the fluid portion of the semen in which they are contained is still a matter of doubt. H. Goodsir|| regards certain albuminous flakes in the fluid portions of the semen of Crustacea as the debris of dissolved cells,

* New System of Anatomy, by JAMES DRAKE, M.D., F.R.S., vol. i. p. 352, 1707.

† TIEDEMANN, Zeitschrift, vol. v. part 2, 1835.

‡ Beitr  ge zur Kenntniss der Geschlechtsverh  ltnisse und der Samen flussigkeit wirbelloser Thiere. Berlin, 1841.

§ Elements of Physiology (Eng. ed.) part 6, 1841, p. 1478.

|| Anatomical and Pathological Researches, 1844, p. 40.

and as the source of nourishment and development of the spermatozoa; while a more recent observer, Dr Kirkes,* regards the spermatozoa as the elaborators of the fluid, and the conveyers of it to the ovum at the time of impregnation. This latter supposition was originally advocated by Wagner, Valentine and Bischoff. But two of these observers have recently changed their views,† and now regard the fluid portion as only of secondary importance in impregnation, and the spermatogenic bodies as of essential. This view, as Wagner states,‡ is founded chiefly on the fact, that in some of the invertebrata the whole mass of the semen appears to be constituted almost or entirely of spermatozoa, while scarcely any *liquor seminis* can be detected;—and further, on the great improbability, perhaps impossibility, of the liquor seminis of those animals which expel their ova into water before impregnation being brought into contact with the ovum. But the same author justly remarks, that “even up to the present day this hypothesis of the influence of the liquor seminis has not met with any direct refutation.” To this I may add, that however strong the presumption may be in favour of the agency of the spermatozoa in those instances in which the liquor seminis has not been observed it affords no sufficient reason for disbelieving that the spermatozoa are not resolved into fluid at the moment of fecundation; or that in those animals in which the liquor seminis occurs in abundance it is not that which impregnates the ovum.

The question then, so far as *proof* is concerned, both of the direct agency of the spermatozoa, and of the non-efficiency of the liquor seminis in impregnation, remains open, as well also as that which involves the knowledge as to how impregnation is effected.

It is to these questions that this communication which I have now the honour of laying before the Royal Society is chiefly directed. I propose *first* to show the time and mode of disappearance of the germininal vesicle, and the condition of the ovum in the Frog and Newt, immediately before and after impregnation, and to endeavour to supply proof from actual experiments that the spermatozoa alone, in all cases of communion of the sexes, are the sole agents in impregnating the ovum; and further, that impregnation *cannot* be effected by the *liquor seminis*; and next to examine in what way the agency of the spermatozoa is influenced, impeded, or exerted.

* Handbook of Physiology, 1848, p. 610.

† BISCHOFF in MÜLLER's Archiv, 1847. WAGNER in Article “Semen,” Cyclopædia of Anatomy and Physiology, part xxxvi., January 1849.

‡ *Loc. cit.* p. 507.

room was occasionally as low as 42° FAHR., yet the creature remained active beneath the water, without relapsing into its previous state of hybernation. It only continued longer beneath the surface without rising to respire. At this period, having found that some frogs in their natural haunts had already come forth, I removed the subject of these observations also from the water to a damp locality, and on the following day found it greatly changed in appearance. While confined in the water it was of a dull dirty brown colour, but some hours after its removal it cast its tegument, and changed to a bright yellow, with the usual brown markings, and had increased in size, both in its body and limbs.

The conclusion to which these circumstances seemed to lead was, that quickly after the frog leaves its hybernaculum, it casts its tegument as the insect escapes from its puparium, and acquires new vigour, while the ova are attaining their full growth. The Toad undergoes a similar change. About a fortnight later in the season than the frog, I have seen many toads in a shallow ditch of slow moving water in the act of casting their dark brown tegument, and acquiring one of a greenish yellow.

On examining several frogs taken from their natural haunts, I found them in, as nearly as possible, the same state of development with regard to the ova, judging from external appearance, as the specimen I had watched through the winter. A few had just paired, but the majority were still single. On opening the bodies of the latter, I found the ovaries greatly enlarged, and the ova, apparently ripe, but still contained in the ovisacs.

It is at this period, therefore, immediately after hybernation, and before the ova have left the ovaries, that the condition of the ovum is a matter of great interest with reference to the structure and contents of the germinal vesicle, the period at which the vesicle is changed or disappears, and the circumstances under which the ovum escapes from the ovary, and is received into the oviduct.

The Germinal Vesicle.—The fate of the germinal vesicle in the matured ovum is still a matter of doubt. Previous to the embryological researches of Dr MARTIN BARRY, it was usually believed that the vesicle entirely disappears before or at the time of fecundation of the ovum. But this view was combated by the author named, who, quoting the opinions of previous inquirers, contended that the germinal vesicle in Mammalia does not disappear, as believed by PURKINJE,* in Birds and Amphibia, by bursting during the generative act, and pouring its contents into the germinal layer of the fecundated ovum.

* Symbolæ ad Ovi Avium Historiam ante Incubationem, 1825; and Article "Ei," Encyclop. Wörterbuch, Band x. p. 112, 1834.

aggregation of white nucleated cells, which, examined by the microscope, exhibited a close resemblance to those seen in the interior of the vesicle. In the midst of these there has occasionally been one or two of larger size than the rest, and which I have imagined to be the remains of the germinal spot, and possibly the origin of the future embryo vesicle of the impregnated ovum, an opinion, however, which I have not had the means of verifying, and I must further state that I have failed to recognise these larger cells in ova that were free in the cavity of the abdomen. Each of the three species of Newt, as well as the Frog, have presented similar appearances in the germinal vesicle and ovum under similar circumstances.

Thus it is quite certain that the germinal vesicle disappears in the Amphibia before the ovum enters the oviduct. I believe it does so in the interior of the yelk, not in the centre, but nearer to the dorsal than to the future ventral or white surface; and not, as has been supposed, on the dorsal or dark surface, between the vitellus and the vitelline membrane. This view is supported by the fact, that that portion of the yelk which incloses the vesicle in an advanced stage of the ovum in the Frog is of a more or less intense black colour, while the vesicle is perfectly white; and that at a further advanced stage, after the vesicle has disappeared, and its place is occupied by a collection of white cells, the dark portion of the yelk still preserves its intense black colour, except at the point that corresponds to the central canal, which then has a leaden hue. PREVOST and DUMAS,* and also RUSCONI,† have mentioned that there is a yellow spot at a corresponding part of the dark surface of the egg of the species they have examined after impregnation, *Rana esculenta*?, but these appearances must not be mistaken for the general vesicle arrived at the surface. There is a similar spot, and that too of an elevated form, on the egg of each of the *Lissotritons*. But independent of the fact that the germinal vesicle has entirely disappeared from the interior of the egg before it escapes from the ovary, this spot is shown not to be the vesicle, both in the fact that in the egg of *Rana temporaria* the dark portion of the yelk is unchanged, while in each case the spot is perforated, and leads into the canal that passed originally to the vesicle. I regard the spot as simply a protrusion outwards of the edges of the canal while closing, after the vesicle has disappeared. I shall presently show that a similar white spot is formed on the under surface of the egg of the Frog soon after deposition, and which might equally well be mistaken for the germinal vesicle.

* *Annales des Sciences Naturelles*, tom ii. p. 104.

† *Developpement de la Grenouille Commune*, 4to. Milan, 1826, p. 9.

are carried forwards to the spaces between the liver and heart on each side to the dilated mouth of the oviduct. They certainly are not seized by the tubes as they escape from the ovaries, as they are constantly found free in the abdominal cavity, while the mouths of the tubes being confined in the peritonæum and having no appendages, cannot be extended to reach them. Their transfer seems to be effected in the Frog in part by the action of the abdominal muscles forcing them onwards in the spaces between the viscera, aided perhaps by the peristaltic action of the stomach and intestines; and their entrance into the tubes, when arrived in the vicinity, seems to be induced by an ingurgitory or suction action at the mouth, occasioned by the alternating and pulsatory motion of the heart, with which the tube is connected by means of the peritonæum. The tube itself is formed of strong longitudinal and transverse fibres, which are continued into the peritonæum, and the former especially into the suspensory ligament, the free external margin of which bounds the outer side of the orifice. The transverse fibres are strongly marked at the commencement of the orifice, where there is a slight pouch; so that when the eggs are entering, these fibres doubtless prevent their return and transfer them onwards.

This, I believe, is the way, in which the eggs enter the oviducts. It is quite certain from the anatomy of the parts, that they cannot be grasped by the oviducts until they are conveyed to them. I have not actually witnessed the passing of the eggs from the abdomen into the ducts in the Frog, but I have seen the eggs moved onwards in the smaller Newt, *Lissotriton palmipes*. Having deprived a female of this species of sensation and power of motion, by division of the spinal cord through the medulla oblongata, I proceeded to open the abdomen to obtain ova from the oviducts for experiments on artificial impregnation. I then found, that a number of ova were free in the abdominal cavity, and that some had very recently entered the ducts, while others were in the immediate vicinity of the mouths. The heart was still pulsating vigorously and with great regularity; and I then saw that at each pulsatory action the ova passed slowly forward between the liver and lung, towards the mouth of the oviduct, which still contained two or three ova that appeared to have entered at the moment of the operation. I did not witness the actual entrance of an ovum, but saw that the action of the heart certainly had the effect of inducing the advance of it to the mouth of the tube, and quite sufficient to lead me to regard this as one of the chief means of its entrance into the duct.

It is not until the ovum has become clothed in the oviduct with its gelatinous envelope that it is susceptible of impregna-

When only a single spot exists, the white surface of the egg for some space around it is more defined than afterwards, and exhibits faint indications of a crucial division of the yelk on this surface immediately around the spot. This is the condition of a few ova immediately after spawning; but the majority have then advanced farther in their changes, and show four or six rounded dark-coloured spots at a little distance in the place of the single central one. When four spots occur they are usually arranged in a quadrangle, and are less than their own diameter apart. They convey the idea of being derived from the central one; but I have never seen any division of this, and if such division takes place, I think it must occur before or at the very moment the ova are expelled. In a further advanced stage of the ovum the four dark spots have become larger, and are each imbedded in a distinct portion of the white surface.

One minute after deposition the spots are more widely separated, and are then each encircled by a separate patch of white substance. *Two minutes* after spawning six dark spots have made their appearance, one of which is situated nearly in the centre, and the remaining five are so arranged around this that the white patches in which they were imbedded seem to have coalesced. In *three minutes* the spots are further enlarged, and appear joined by a dark line of colour extending from each, so that the whole form, as it were, a knotted ring that includes a patch of the white surface of the yelk with one of the dark spots near the centre. In *four minutes* the ring around the included white substance is more distinct, and the white surface of the egg has increased in extent. In a further advanced stage at this period the white portion included in the ring exhibits the appearance of a white, very opaque patch, the dark spot in the centre having disappeared. Around this opaque white patch is the dark-coloured knotted ring, now become more uniform, and resembling a ragged chink or slight circular furrow or division in the white surface. The centre of this hemisphere of the egg thus comes to be occupied by a white patch instead of the dark spot. At *five minutes* this central white patch,—which, as before stated, and from what afterwards occurs, may readily be mistaken, on casual inspection, for the germinal vesicle, altered in its appearance and arrived at the surface,—were it not that we now know that this has long before entirely disappeared,—becomes more defined, and the dark circle around it is more uniform and distinct. At *ten minutes* the central patch is a little reduced in size, and the circle that encloses it begins to take the appearance of a diffused halo. At *fifteen minutes* the central white patch is more reduced, and the halo is spread wider, while the whole of this

centre of this surface of the yelk. *One hour* after spawning this depression is somewhat deeper. The white surface has become still more defined, and the dark has acquired a more intensely black colour. The egg remains in this state without further perceptible change during the succeeding *second* and *third* hour, excepting only that the depression in the white surface becomes a little deeper, but it has almost disappeared at the end of the *fourth* hour, when *segmentation* or cleavage of the yelk is about to take place. But this is not invariably the case. When it does remain it is always of an oval form, and the primary cleavage of the yelk, as it proceeds on either side, from above downwards, meets in its centre and invariably passes through it transversely to its long diameter. These are the first perceptible changes in ova that are impregnated by the natural union of the sexes, and when spawning has not been retarded. But in some broods of eggs that have been retained longer than usual in the oviducts, the whole of these changes have already taken place, in so far as regards those of the yelk, the white surface of which then exhibits an uniform appearance.

Changes immediately before segmentation or cleavage of the yelk.—Segmentation usually commences in from four to five hours. At about *one hour and a half* after spawning, the peripheral layer of cells on the middle of the dark or uppermost portion of the yelk of the impregnated ovum becomes separated from the inner surface of the vitelline membrane, and this separation goes on until a broad free space is left between this envelope and the superior layer of yelk-cells. This space, which we may designate the *respiratory chamber*, is at first but a small area above the middle of the dark surface of the yelk, and is commenced above the central canal. It seems to be occasioned by a recedence towards the interior, or a shrinking, at this period, of the yelk cells of the dark hemisphere of the egg, commencing in the centre of this part and extending gradually, but in a less degree, to the circumference. This recedence goes on until the space left between the vitelline membrane and the yelk is equal to about one-sixth of the diameter of the whole mass, when the space appears to be occupied by a very transparent fluid, interposed between the now depressed surface of the yelk and the vitelline membrane. In the centre of the black surface is the minute orifice noticed by PREVOST and DUMAS,* and BAER,† which leads into the central canal that communicated with the germinal vesicle in the ovarian ovum. It is in the margin of this canal that segmentation is commenced. While the space or chamber between the black

* *Loc. cit.* Tom. ii. p. 104, 1824.

† MÜLLER'S Archiv, 1834.

expand, it is then equal to about one-fourth of the diameter of the yelk. At the end of *two minutes* it is enlarged to one-third, and in *three minutes* to one-half the diameter of this body. In *four minutes* it exceeds three fifths, and in *six minutes* two-thirds, and it continues to imbibe fluid and expand at the same rate, until, at from *ten to fifteen minutes*, it very nearly equals in thickness the whole diameter of the yelk; and at *half an hour* it is one-fourth greater than this. PREVOST and DUMAS* noticed the expansion of the envelope during the first *six hours*, but entirely overlooked the rate of expansion during the most important period, the first hour, and noticed only the general fact that the diameter of the envelope, at the end of the first hour and a half, was as 5 to 2·5 at the time of spawning, and that it had nearly acquired its full size at the end of three hours. My own observations agree with this latter statement. The expansion of the envelope is greatly retarded at the end of the third or fourth hour, until after cleavage of the yelk has taken place, when it again proceeds, but much more slowly than at first. If then we bear in mind the rate of expansion of the envelope during the first half-hour, the following experiments will give some idea of the degree of susceptibility of the ovum to become impregnated during that period.

Mr Newport subsequently, on the 6th of April 1850, performed a series of six experiments, in which, after immersing in water, *ova* varying in number from eighty-three (83) to two hundred and five (205), and added impregnating seminal fluid, so as completely to cover the ova; and two experiments in which eighty-one ova, and one hundred and thirty nine ova (139), were exposed to dry air, and then subjected to the operation of seminal fluid applied in an effectual manner.

Of this series of experiments the following tabular summary indicates the results:—

TABLE I.—Set F.

Experiment.	Ovs.	Time.	Medium.	Fluid obtained.	Segmentation.	Embryos.	Per centage.
		"		"	" "		
No. 1	83	1	Water.	43	3 55	32	·38
No. 2	92	2	Water.	43	3 55	45	·49
No. 3	127	3	Water.	17	4 3	33	·26
No. 4	81	3	Air.	8	4 5	53	·65·5
No. 5	136	5	Water.	20	4 8	10	·07
No. 6	139	5	Air.	12	4 11	37	·26
No. 7	205	15	Water.	26	4 14	45	·22
No. 8	100	30	Water.	44	2	·02

* *Loc. cit.*, vol. ii. p. 108.

disinclination to walk seemed to be due, at least, as much to her nervous temperament and wayward disposition, as to any physical incapacity for exertion.

She was in the fifth month of her pregnancy when she placed herself under the care of Mr Wren, of Brownlow Street, to whose kindness I am indebted for many facts in her history, as well as for the permission to lay her case before this Society. There was nothing in her appearance, either when she first came under Mr Wren's care or subsequently, to suggest the idea of her being deformed; the only peculiarity which she presented being that she always stooped very much forwards. During her pregnancy her bowels required the constant use of purgatives; her appetite was bad, she was much annoyed by heartburn, had occasional faintings, and suffered much from palpitation; her pulse beating habitually 120 in the minute, (a rate of frequency which she said it had manifested for years), but varying under every source of excitement. She was nervous and excitable in the highest degree, looking forward with much apprehension to her labour, and especially to the pain attending it; and was urgent in extorting a promise that she should be permitted to inhale chloroform when labour came on.

It was in this unfavourable state, both of body and mind, though much benefited by Mr Wren's treatment, that Mrs Williams reached the end of her pregnancy, and labour pains came on at 1 A. M. on the 7th of May 1850; she having been married just two years, and being in the 27th year of her age.

Mr Wren was summoned at 3 A. M., and discovering, on his first examination, the existence of extreme pelvic deformity, despatched a messenger for me, and I arrived at a quarter to 5 A. M. I learned that the pains which the patient had had were very feeble, and returned only about every quarter of an hour. She bore them, however, very ill, tossing about the bed, crying out for chloroform, and becoming almost unmanageable on any attempt to make a vaginal examination.

She was a small, slender person, and, as she lay in bed, presented no sign of deformity, and her extremities were straight and well formed. Her face was pale, and her pulse frequent and feeble. On examining her spine, it was found to be perfectly straight, but the sacrum was bent into an almost semicircular form, with a great convexity projecting backwards; the pubic arch was a little wider than would suffice to allow of laying one finger between the rami of the pubes; the tuberosities of the ischia did not seem to be much above one inch apart, and the rami of the pubes ran out into a sort of beak; the bones being bent at the junction of the pubis and ischium, and being at this point not more than one inch apart.

cation of returning consciousness was furnished by several violent attempts to vomit, during which the intestines (especially on the left side of the uterus), which had come into view immediately on the removal of the child, prolapsed, and could with difficulty be retained in the abdomen, even by closing the integuments over it.

By degrees the hæmorrhage abated ; no other means having been resorted to, to excite uterine action, than moderate pressure with the hand on the abdomen. At 6 p.m., the hæmorrhage had quite ceased, though the uterus was not firmly contracted, nor the wound closed tight. Five sutures were now placed in the abdominal integuments, the last, one inch and a half above the lower edge of the wound ; a space being left here at my request, in compliance with the suggestion of some continental surgeons, who attach importance to an aperture being left for the escape of the discharges from the uterine wound. Broad strips of plaster were next applied ; and an eighteen-tailed bandage, which had been placed under the patient before the operation was begun, was now brought together over some large sheets of cotton wool, which served to protect the abdomen from pressure.

The patient was now carefully lifted into bed ; and immediately on being placed there, efforts at vomiting came on, during which firm pressure was made on her abdomen, to prevent, if possible, protrusion of the intestines. As soon as they had a little subsided, sixty drops of tincture of opium were given in a little brandy, but were almost immediately rejected, and she refused to take any more laudanum, saying it would make her vomit.

At this time, though her pulse was extremely feeble and her extremities were cold, yet her mind was calm and collected ; nor did her intellect become at all disturbed, until within an hour or two of her death.

Two doses of morphia were given her between 7 and 10 p.m., but both were rejected soon afterwards, and the patient made frequent efforts to vomit during the first two hours after the operation. At 10 p.m., however, she expressed herself as feeling comfortable, and her pulse, though extremely feeble, did not exceed 108 beats in the minute. On examining the wound, a portion of omentum was seen to have escaped beneath the lowest suture, and having been returned with some difficulty, an additional suture was put in to close the lower edge of the wound, and the renewal of the accident was thus prevented.

In consequence of the irritability of her stomach, it was determined to endeavour to keep the patient under the influence

ter had returned, and the patient seemed more feeble, She had taken very little by the mouth, and the disposition to sickness and distaste for almost everything, with the inability to bear more than a teaspoonful or two of any nourishment at a time, interfered greatly with all endeavours to support her powers. She had complained sometimes of headache, especially when sickness was felt; and it became a question with Mr Skey and myself whether any advantage likely to accrue from the continuance of the laudanum in the enemata was such as to counterbalance the possible evil of maintaining a constipated condition of the bowels, and of keeping up or aggravating the patient's sickness. The character of the matters vomited seemed, indeed, to point to a more serious and less remediable cause; but still it was determined to try the effect of omitting the laudanum.

Various stimulants and various articles of food were tried during the course of the day, but she took very little of any; champagne and ice being the two things that seemed most grateful to her. She had one or two returns of green vomiting during the day, but did not appear, at 11 P. M., to have at all retrograded since the morning.

She became very restless, however, and was sick twice after 11 P. M.; the opium was accordingly resumed in the enemata, and early on the morning of May the 11th, small doses of the black drop were given every two hours at Dr Murphy's suggestion. By 3 P. M., under the continuance of the opium and the enemata of beef-tea, with such small amounts of food and stimulants as she could be induced to take, she rallied surprisingly; the lochial discharge, which had almost ceased for the previous twelve hours, reappeared; and her pulse, which had ranged at about 140 to 150, though still 146, was somewhat less feeble.

At 3 P. M., Dr Ramsbotham, Dr Murphy, Mr Skey, Mr Wren, and I, saw her together, and felt more hopeful about her than at any time for the previous twenty-four hours. There was, however, one bad symptom about her even then, which was, that her temperature, which during the night had sunk very low, continued so, and that there was a cold perspiration upon her surface. Almost immediately after this visit, too, the other favourable symptoms began to disappear; her skin became quite cold, her pulse thready, and she sank into a state of collapse like that of a cholera patient, her intellect continuing clear, and considerable muscular power remaining, until within a couple of hours of her death, which took place at half past 6 A. M., on May 12th, 108½ hours after the operation.

processes which nature is prepared to carry on in it, consist in the desintegration and removal of its tissue; the very opposite, indeed, to those essential for the repair of injury.

To all, except the last of these sources of danger, attention has been more or less directed; but still the amount of peril to which the patient is exposed from each, has, perhaps, been scarcely sufficiently investigated.

Kayser, in his valuable essay on the Cæsarean Section, states the cause of death in 123 cases; in 77 of which the patient died of inflammation, in 30 from the shock to the nervous system, in 12 from hæmorrhage, and in 4 from some accidental occurrence not of necessity associated with the operation. In some of these cases, however, no post-mortem examination was made; in others the date of the patient's death is not stated; while in several, more than one of the above-mentioned causes had contributed to occasion the patient's death. Selecting, therefore, such of Kayser's cases as were complete in all respects, and adding to them such others as have since come to my knowledge, with the requisite details, I have drawn up the accompanying table as affording some approximation to a correct view of the causes of death after the Cæsarean section.

From this table* it appears that, in 41 out of 174 cases, a notable amount of hæmorrhage occurred either during the operation or subsequent to it; and in a third of this number hæmorrhage was the sole cause of the patient's death. In seven instances the hæmorrhage arose, in great measure, from the placenta being wounded in the course of the operation; an accident which probably might be avoided in the majority of instances, by careful preliminary auscultation. In twenty instances it occurred at the time of the operation, and proceeded in part from the edges of the wound, in part, and usually in greater measure, from the seat of the placenta, and followed its detachment or spontaneous separation. In the remaining fourteen cases, the more important bleeding was secondary, taking place after the completion of the operation and closure of the wound, escaping externally in one or two instances, but in the other cases being poured out into the abdominal cavity, and being discovered in the form of coagula of greater or less magnitude, on examination of the patient's body after death.

Against this hæmorrhage, the resources of art can effect but little. The injury of the uterine substance can hardly fail to impair the contractile power of the organ, while that arrangement of the uterine sinuses which tends to prevent the occurrence of hæmorrhage after the separation of the placenta, even

* *Vide*, p. 403.

inflammation of the peritoneum, there is the absolute necessity of the occurrence of some degree of inflammation for the closure of the wound, and the repair, in as far as nature can effect it, of the grievous injury which has been inflicted by the operation. How narrow must be the limits which, in a patient who has undergone the Cæsarean section, separate the healthy action essential to repair, from the morbid action that tends to destruction !

The state of the wound has, unfortunately, engaged less attention than the interest and importance of the inquiries on which it bears, might well demand. Still, even from the imperfect data with which we are furnished, it seems clear that one of the first steps which nature takes towards the repair of the injury in these cases, consists almost invariably in the establishment of inflammation of the peritoneal surface of the uterus, and of the corresponding portion of the abdominal peritoneum, or, less often, of the peritoneal surface of the intestines, with the view of forming adhesions all round the wound of the uterus, and thus isolating it from the cavity of the abdomen. This being effected, adhesion takes place between the cut edges of the peritoneum, which, lymph effused upon its surface, strengthens, though this often takes place but slowly, and, for a long time, imperfectly ; as, for instance, in a case related by Professor von Ritgen,* in which a fistulous communication between the surface of the body and the interior of the womb did not close till the sixty-fifth day. Afterwards the divided tissue of the uterus itself, in some instances, becomes united, though this last step always takes place most imperfectly, and sometimes not at all.

In many of the fatal cases it has been found that no step whatever has been taken towards repair ; in more the process set up has been a perverted one, and there is no attempt at adhesion ; but a dirty exudation covers the pale and bloodless peritoneum extensively, or is poured out in a fluid form into the abdominal cavity ; while in others, as already mentioned, parts are found with their vitality destroyed, and this not by the excessive activity of powers which, moderated, might have conduced to repair, but by their deficiency.

And this brings me to the last cause of the high mortality which follows this operation, and a cause against which skill can avail absolutely nothing, since it is inseparable from those processes which nature establishes after the uterus is emptied of its contents, be the period of pregnancy, at which that takes place, what it may. In a large proportion of cases, the record

* *Neu Zeitschrift für Geburtskunde*, Bd. ix. Heft ii.

multaneously with the removal of the old tissue, there goes on a formation of new; but of a tissue much more lowly organized, possessed of but small power to repair injury, and which commonly requires, for its higher development, the appropriate stimulus furnished to the uterus by pregnancy, and the sojourn of an ovum in its cavity.

The condition of the uterus in the patient whose history I have related, gave an apt illustration of the correctness of these views. The organ was generally pale and bloodless, the edges of the incision through it were swollen, infiltrated, œdematous, and discoloured by altered blood, so as to look, at first, almost as if gangrenous; and I have little doubt but that, in many of the cases in which the organ was alleged to have been in a state of gangrene,* that condition was, as in the above instance, apparent rather than real.

If, then, such and so many dangers beset this operation, if the recoveries from it be so few, and the mortality so great, while the causes of that mortality are, for the most part, beyond the power either of surgical dexterity or medical skill to obviate, and some of them inseparable from those processes which needs must follow delivery, we may, I think, feel satisfied that the general rule in British Midwifery, which prohibits the performance of the Cæsarean Section, except where delivery would otherwise be altogether impossible, rests on a far sounder foundation than that of mere prejudice, or blind obedience to the dicta of men eminent in their profession.

At the same time, these facts can yield no apology for those who, in cases calling for this operation, allow, from any cause whatever, the right moment to pass by unused. They rather leave such conduct altogether without excuse, since, by it, dangers always great are rendered insurmountable, and hopes, small at the best, are utterly destroyed.

* Many cases of an analogous condition of the uterus, in women who have undergone the Cæsarean section, might be adduced. The following may suffice from the history of a woman who died thirty days after the performance of the Cæsarean section. "The wound of the uterus was slightly closed at its lower part; but at its upper end, where it had been carried quite up to the fundus uteri, it not merely gaped widely, but the absorption of its edges had gone on with such activity, that nearly the whole of the fundus uteri was consumed."—Busch, *Geburtshülfliche Abhandlungen*, 8vo, Marburg, 1826, p. 243. He attributes this condition of the uterus to the effects of the osteomalacia, which was still advancing at the time of the patient's death: but it may, I think, with more propriety, be referred to those causes mentioned in the text as acting on the uterus after delivery.

TABLE (referred to at p. 396.)
Showing the Cause of Death, and the Date of its occurrence, in 147 Ruled Cases of the Caesarean Section.

Date of Death.	Hemorrhage.		Shock to nervous system.		Inflammation.		Hemorrhage and shock.		Hemorrhage and inflammation.		Shock and inflammation.		Causes independent of operation.		Total.
	No. of cases.	Reference.	No. of cases.	Reference.	No. of cases.	Reference.	No. of cases.	Reference.	No. of cases.	Reference.	No. of cases.	Reference.	No. of cases.	Reference.	
Under 1 hour.....	1	1	1	15*	2
Between 1 and 2...	2	2-3	1	15	2	101-2	6
" 2 " 6...	2	4	47	2	2
" 6 " 12...	1	5-8	7	16-22	1	48	...	102-4	2
" 12 " 24...	4	9-10	7	23-28	1	49	2	14
" 24 " 36...	2	11	6	29-33	11	61-60	23
" 36 " 48...	1	12-13	5	34-40	7	61-67	19
" 48 " 72...	2	...	7	41-44	0	68-74	2	105-6	4	115-8	2	132-3	24
" 72 " 96...	4	75-82	1	107	2	119-20	1	134	18
On 5th day	1	14	1	45	5	83-87	2	108-9	4	121-4	3	135-7	16
" 6th "	1	46	2	88-89	1	125	1	138	5
" 7th "	4	90	1	140	2
" 8th "	4	91-94	1	141	7
" 9th "	2	...	2	96-96	2	126-7	1	142	3
" 10th "
Between 10th and 12th	2	97-78
" 12th " 13th	1	99	2
" 15th " 20th	1	100	1
Above 20.....	2	143-4	3
Total.....	14	...	88	...	66	...	9	...	18	...	11	...	6	...	147

patient lay on her side, on account of the height at which it was placed, the extreme tenderness of her soft structures, and her inability to flex her thighs well, which was caused by the prominence of the anteverted uterus. She was therefore placed on her back, and the canula having entered the amnial sac, about eight ounces of clear *liquor amnii* were drawn off. At this time the foetal head could be felt, distinctly marked out on the abdomen above the pubes.

24th. This morning the bowels had been again relieved by aperient medicine, and some uneasiness about the uterus had been felt. Mr Davies, who had charge of the case, examined and found the left arm in the vagina. On visiting her, I found that the child was still alive, and that touching the palm of the hand in the vagina, excited reflex muscular movement. The *os uteri* surrounded the arm, and the uterus was quiescent. The patient was ordered to keep quiet, and it was requested that no further vaginal examination should be made.

25th. She had not slept well on account of some irregular diffused uterine pain. The arm and *os uteri* were in the same condition. She had passed urine freely, and during the day was tolerably free from pain. Her pulse is rapid, 93 in the minute, which is natural to her; but there are no febrile symptoms, and she is in good spirits.

26th. Early this morning 6 drachms of castor-oil were given, which acted freely on the bowels in the course of the morning. At 9 a.m., some regular uterine pains commenced; and when I saw her the *os uteri* was found to be dilating well, and the vagina softer and less tender. The skin of the foetal arm was peeling off.

Vaginal examination was forbidden.

At half-past 9 p.m., the *os uteri* was fully dilated, but the loose soft margin of it could still be felt, on account of the foetal head being so held above the brim as not to have any bearing upon it. Some efforts at delivery were now made, the rectum and bladder being both empty.

A full exploration of the pelvic brim and the presenting part was made by the cautious introduction of the fingers of the left hand, with a view, if practicable, of running them beyond the brim, and catching and bringing down a foot; the right hand at the same time pressing the uterus from the abdomen, and doubling down towards the pelvic inlet the lower limbs of the fœtus, in a direction to meet the hand in the vagina. The patient was at this time on her back, but the pelvic brim was so contracted that it was soon obvious, that any hope of delivery in this way must be abandoned. Traction was then made on the protruding arm, which shortly gave way at the elbow-joint; but the

conmode Mr Poland. The pulse was of good strength, and she was left reclining on her back, and sleeping in a tranquil manner under the influence of chloroform.

At a quarter to four, A.M. three quarters of an hour after the operation, she opened her eyes as though waking from a natural sleep, and she then told me that she had known nothing of the operation, and felt quite comfortable. Just before this her pulse was 118 in the minute, and the respiration 32.

During the remainder of the night she took two drachms of laudanum; at first, one drachm was given her soon after she awoke, half an hour after which she began to shiver; the pulse became rapid and weak; the respiration quickened and catching; the extremities cold; and she complained urgently of an oppressive tightness over the upper part of the abdomen. A small quantity of hot brandy-and-water, and half a drachm of laudanum were given; and some warm clothes were placed around her. The bandage was loosened at its upper part, which gave her some relief. In an hour's time she was in a profuse sweat, and felt easy, but was indisposed to sleep. The remaining half drachm of laudanum was then given, and towards morning she had some sleep.

First day.—In the morning she expressed herself as feeling comfortable, and free from pain. She breathes easily, and the skin is freely perspiring. Pulse 130; tongue moist, slightly furred. The upper part of the abdomen, above the bandage, is distended with flatus, so as to bulge up above the edge of the bandage. Six ounces of dark coloured urine were drawn off by the catheter. The pupils are contracted, evidently from the effects of opium, and she is in a quiet drowsy state, readily, however, answering questions, and quite free from restlessness.

Great care was taken to keep her undisturbed, and she was ordered cold drinks of spring or toast-water, or tea. In the course of the day she was ordered—*Julepium Ammon. Acet. c. Mist. Camph. et. Tinct. Hyoscyam. quâque 3ss. 4tâ horâ.* But of this she took only two doses.

In the evening.—She had passed the day in a perfectly tranquil manner, sleeping a good deal. She had taken freely of the cold liquids from time to time, and there was but little alteration in her general state, which was as favourable as could be expected. Between six and seven ounces of clear urine were drawn off. She had sweated profusely at intervals during the day, requiring the clothes to be lightened, and now the skin is warm and moist. There is no abdominal tenderness.

The vulva was sponged with warm water; warm napkins were applied, and the sheets were shifted from under her with as little movement as possible. There was a moderate dark-coloured lo-

of viability, there was no expectation of saving the child, but only that, by removing a body of relatively small dimensions, the operation by the crotchet would be facilitated. The important favourable circumstance in this mode of delivery was, that it afforded, in my judgment, a reasonable expectation of being completed through the birth-passages by an operation, from which the mother might escape with impunity, although at the sacrifice of the child.

In favour of the Cæsarian section, there was the probable preservation of the child, and the somewhat diminished risk from so formidable an operation, by a careful preparation of the patient for it;—undertaking it at a selected moment, and with such attention to points of practice, during and after it, as have lately been found of service in abdominal surgery. This mode of delivery would, I believe, have found favour in the eyes of some obstetric practitioners, with whom the expectation of saving the child's life would have outweighed the degree of operative danger to which the mother would be exposed. My own judgment was clearly in concurrence with the practice which was adopted. Had the pelvic deformity been greater, and pregnancy, as in this case, been concealed until the seventh month, preventing, therefore, an early induction of labour, there could have been no hesitation in having recourse to the Cæsarean section. But with a conjugate diameter of two inches, and a sufficiently spacious cavity and outlet, there was so fair a likelihood of delivering by the crotchet, that I should have felt culpable in not undertaking it. In the election between the safety of the child, and an augmented danger to the mother from a serious surgical operation, I was not disposed, in this case, to tamper with the maxim which happily prevails with obstetric practitioners in England in favour of the mother. And although the results of this case, when separated from these and after considerations, and appearing nakedly as in a statistical table, are most gloomy and disappointing, and delivery by the Cæsarean section at first, might have rescued the child, and offered a better chance for the mother, yet I should not, in a similar case feel justified in deviating from the same practice. My conviction is, that the mal-position of the child, which was, of course, unexpected, and beyond control, so augmented the difficulties of the first hindrance, so as to thwart my purpose.

(2.) With reference to the second question, it will be noticed, that twelve hours were suffered to elapse, to allow the first stage of labour to be well completed, and to favour the physiological softening of the vagina and external organs. In introducing my hand, with the view of learning more accurately the degree of deformity of the pelvis, I had the remote expectation that I might probably seize, with the fingers, a lower extremity, by the same kind of manipulation which Dr Lee has practised successfully in

which also failed. The pelvis was found to be contracted in all its diameters, and the conjugate diameter measured two inches ten lines. Eight hours from the first attempt at delivery, she was removed from her lodgings to a ward prepared for her in the hospital, and Dr Ashwell then took charge of the case. She had already had a rigor; the pulse was rapid, and the abdomen tender. Without trying again to turn, Dr Ashwell made attempts to remove the child by the cutting hook; but it could not be fixed upon the neck to sever the head from the trunk. The thorax was perforated; but after the most strenuous efforts, perseveringly followed out for many hours, it was obliged to be abandoned as hopeless, and the woman died undelivered. The pelvis is now upon the table. "If this case could again occur," says Dr Ashwell, (*vide* Guy's Hospital Reports, Vol. i,) "at the full period, and the knowledge now attained were possessed, there would, I think, be little hesitation as to the propriety of the Cæsarean section."

Another unfavourable circumstance in this case, in reference to persisting with the crotchet, was the state of the vagina. This canal has been noticed as retaining the structure of early age. It was closely ribbed throughout, very vascular, with a thin epithelial covering, and a feeling imparted to the finger of its being structurally weak and easily lacerable. This was remarked to those around me at the beginning, as a source of danger in a prolonged craniotomy case; and, excepting when the hand was introduced which lacerated the fourchette, this canal was very carefully guarded when the crotchet was being used. Should the patient survive the immediate dangers of delivery, I could not help feeling that there was a great risk of vaginal sloughing, with its revolting consequences.

II. On the other hand, the Cæsarean section offered a speedy and sure, instead of a prolonged and doubtful delivery. It was not yet forbidden from exhaustion or any signs of inflammation; on the score of suffering, it contrasted most favourably with the other practice, and I thought, upon the whole, that the chance of ultimate success from it was greater.

The operation was performed much in the usual way without difficulty, and with nothing untoward. The extent of the reparation of the external wound, and the admirable manner in which the structures were laid out, to limit the mischief from the open wound in the uterus, and prevent its extravasated contents from being diffused in the abdominal cavity, are worthy of attention, and must have been greatly favoured by the perfect tranquillity of the patient under the influence of opium.

this difficulty, at the outset, Dr Ashwell defines a functional disease to be one *which is dependent on deviation from the natural or healthy action of any part of the organization, indicated by symptoms during life, which, on examination after death, are found to be unconnected with any discernible change in structure.*

Functional or Dynamic Uterine affections, Dr Ashwell observes, are mainly dependent on the derangement of menstruation; such, for instance are Chlorosis, Amenorrhoea, and Dysmenorrhoea, profuse Menstruation or Menorrhagia, and in some instances Leucorrhoea. In all these disorders a deviation from the standard of uterine health is observed to take place. These deviations are evinced in the absence, the scantiness, the excess of the uterine secretion, or in the substitution of a mucous, a muco-gelatinous, or a muco-purulent secretion instead of the normal one.

Dr Ashwell states it as the result of his observation, that, in judging of these deviations, it is less important to attend either to the amount or the quality of the uterine secretion, than to the regularity in the periods at which it takes place.

In estimating the influence of the state of this secretion upon the health of the individual, he adheres to the opinion that it is peculiar to the human race, and denies entirely the truth of the statement maintained by some, that a similar secretion takes place in the females of certain of the ape tribe.

With the affections now mentioned structural changes are not necessarily connected. Excepting the undeveloped condition of uterus and ovaries in chlorosis, anatomical inspection after death does not present any change, sufficient to account for the derangements often considerable in the health of females suffering under these disorders.

Even in the case of protracted and incurable Menorrhagia, Dr Ashwell observes, which occasionally terminates life by giving rise to dropsy and similar secondary diseases, if there have been no morbid growths co-existing, the uterus yields not any evidence of much structural disease. Increased softness and paleness of the uterine parenchyma and lining membrane will generally comprise the whole of the visible organic change. The same may be said of Amenorrhoea and Dysmenorrhoea. Beyond Hypertrophy and congestion in the latter affection, the organization generally continues unaltered.

It seems scarcely reasonable to admit, that the presence of the conditions last mentioned are competent to prove entirely the position of the author, that there is no organic change. Anatomists and pathologists are not in all instances well agreed as to what constitutes an organic change. But we suppose that not a few would say that hypertrophy is a certain degree of change

and softer—if the cough be less frequent—if the pyrexia disappear—and especially, if the patient gather flesh, in ever so trivial a degree—hope may be entertained.

“Carefully treat such an individual ; avoid mercury, drastic purgatives, and emmenagogues. Place her in the country, where she shall breathe pure air : let her diet be simple and nutritious (milk and animal food), and her medicine some of the various tonics ; and the expectation may be cherished, that the time is not far distant, when the sexual character will be fully developed, and the danger safely passed. From what has been stated, it must not be inferred, this is the only fatal complication of chlorosis ; but, comparatively, that it is rare for the others to terminate unfavourably. Still, after continued derangement of the viscera of nutrition and digestion, the debility, pyrexia, and emaciation may become intimately blended with alteration of the pulmonary structure ; and the cough, expectoration, and morning perspirations, may become prominently influential in bringing about final sinking.

“I have thus attempted to distinguish the morbid circumstances appertaining to the different forms of this prevalent malady. And although the leading and distinctive features will generally enable us to determine the complication ; still, when any form of the disease has become aggravated, severe and of long duration, the blending of symptoms may perplex the diagnosis. It ought to be remarked, that leucorrhœa, in various degrees, is an almost constant attendant on chlorosis and amenorrhœa ; and, when excessive, so seriously impairs the restorative powers, as to render the cure long and difficult.

“I wish to particularise, as correctly as I can, the ages, at which these various complications most frequently occur. Chlorosis alone, independently of amenorrhœa, is a disease of early life. In conjunction with menstrual suspension, it may be met with at any period, between the ages of puberty and the final cessation of the catamenia. Chlorosis conjoined with phthisis, may be seen between puberty and thirty years of age ; sometimes later ; but such instances are very rare, in comparison with the numerous complications of this kind, before the attainment of the twentieth year. Again, chlorosis with amenorrhœa or phthisis, at an early age, are forms of the malady, generally associated with debility and delicacy of system ; while the other complications may exist at any period, and are not unfrequently combined with plethora or congestion.”—Pp. 22, 23.

It is impossible to doubt the frequency with which this complication takes place, and the difficulty that in certain cases arises of distinguishing it aright and applying the proper treatment. This difficulty is considerably increased in consequence of the fact, that phthisis itself, not only in its approach, assumes something of the appearance of chlorosis, but is often preceded by amenorrhœa, or gives rise to that condition. Conversely, in all chlorotic cases, there is more or less dyspnoea very similar to that of phthisis, and not unusually cough. Indeed, the two affections

so closely resemble each other in certain females, especially about the time of puberty, or shortly after that period, that it is often difficult to say, whether any given case is one of pure chlorosis or pure phthisis primarily, or of chlorosis threatening to proceed to phthisis, or of phthisis giving rise to chlorotic symptoms. Physicians of considerable skill and experience we have seen prescribe the use of iron in phthisis, upon the idea that the case was a chlorotic one or a chlorotic complication; whereas, had they been quite certain it was not, they would probably have withheld the mineral, and trusted to remedies less calculated to induce vascular irritation and excitement.

While the importance now stated both as to frequency of occurrence and the danger of the complication must unquestionably be conceded to this pulmonary form of the disease, it must not be kept out of view, that the fourth complication, namely, that in which the nervous system is more or less affected, is also one of no uncommon occurrence in certain classes, and one of very great importance as affects the subsequent health of the individual. It is in different classes of society produced in different modes.

Among the more easy classes it is probably induced by neglect of proper education, both physical and moral, by habits of corporeal inactivity, late hours, and many of the customs and rules observed in young ladies' boarding-schools and similar seminaries. The great desire at present, and for many years past, to cultivate the mind, and acquire accomplishments by incessant labour at music, drawing, and similar pursuits, carried on within apartments not always very well ventilated, at the expense and to the detriment of the corporeal health, manifestly contributes in a very decided manner to induce one or other of the forms of this complication. The mind and fingers are over-worked; and the body is inadequately exercised.

Among the working and inferior classes, again, it is incessant hard labour, scanty and innutritious food, with late hours, and want of sleep, or at least too short a period allowed for repairing the exhaustion consequent on the toil of the day, that not unfrequently are the antecedents of one or other form of this complication. In large, and populous, and middle-sized towns, there is a class of domestic female servants, who are not unusually victims of this complication, or some similar disease, produced in the same manner. The single servant of all-work, in a large or small family, with not very large means, has indeed a hard and miserable form of existence. She is expected to work the whole day, in various duties, continuous and laborious, and to extend her labours far into the night or early morning, in washing, or some other occupation, often on very scanty and inadequate fare; and then retiring to rest, she is understood and compelled to rise after a few hours sleep; and day

and ignorant attempt of prematurely establishing menstruation ; mercury, drastic purgatives and emmenagogues, having irretrievably destroyed the constitutional power and paved the way for phthisical disease.

"It is not my intention elaborately to comment upon certain great mistakes in the *physical education* of female youth, And yet, I must be excused, if I direct attention to the diet, air, exercise, and clothing of the sex. It will readily be granted, that if, in these particulars, there is extensive deviation from the dictates of nature and common sense, there must be a proportionate risk of debility and disease. In our own changeable climate, it behoves the guardian of female youth to be especially prudent ; and I am one of those who think, that it is scarcely possible to study these matters too closely. If the national practices in these particulars could be changed—and the remark applies with great force to the middle and higher classes of society living in cities and towns—chlorosis, imperfect puberty and amenorrhœa, would be uncommon, instead of being, as they are now, extremely prevalent diseases.

"Chlorosis is a rare affection in rural districts ; where female youth are much in the open air, where it is not unfashionable to walk and run, and where it is not considered a gross violation of good breeding to sport and play with activity and vigour. Such girls acquire energy of system, each organ is developed, the blood is abundant and of excellent quality ; nutrition is healthy, and puberty is attained without difficulty.

"These remarks may serve as an illustration of the principles on which the treatment of simple and amenorrhœal chlorosis must be conducted : and while it is scarcely possible to present a succinct and specific history of the pathology of this and the other complications, it is not difficult to describe, with simplicity and tolerable accuracy, the order of morbid events and the medicinal means, by which they are to be relieved if not cured.

"I have already observed, that a morbid state of the blood, of which anæmia is the prominent feature, lies at the basis of the disease. This may be viewed as the clue, by which the intricacy of the symptoms may be unravelled ; and it will equally explain the nature of the malady, whether the specific morbid impression be in the system generally, in an isolated organ, a particular texture, or in any of the fluids of the animal economy.

"But, to be more precise. I would commence the treatment, by special attention to the digestive organs and alimentary canal ; for I regard the disorder of these, as second only in pernicious effect, to the peculiarity of constitution, already mentioned. Nor will the advantage of their improved condition be limited to themselves ; the deteriorated quality of the blood and its defective quantity, may both owe their origin to impaired digestion and nutrition. I have already alluded to the jaundiced hue of the complexion and of the surface generally, as leading to the suspicion of hepatic disease. The diagnosis will be made, by a careful examination of the region of the liver itself, of the urine and the *feces* ; which will prevent

varieties ; 1. Recent and acute suppression and of chronic suppression.

On the first form and variety, or that dependent on congenital deficiency, malformation or structural disease of the organs of generation, Dr Ashwell remarks that it is now almost universally acknowledged that menstruation, as well as conception, depends on the existence and influence of the ovaries. In instances, therefore, in which these organs are either wanting, or considerably diseased, the case is irremediable.

“ The history of these cases is not encouraging : the health often suffers, and there is a proneness, either to irritability and excitement, or torpor and depression. I have now under my occasional care, a lady of thirty-two years of age, who has never menstruated, I believe, from congenital deficiency of the ovaries, and she is never quite well. Of late, her health has been more seriously deranged ; she loses flesh, has frequent febrile attacks, a troublesome cough, pain in the side, and embarrassed respiration. The probable termination of this unhappy condition is phthisis. In this instance, sexual development and feeling are entirely absent ; nor has there ever been leucorrhœal discharge.”—P. 57.

It is well known that in cases of this kind the health is much affected. The sexual peculiarities of the patient often disappear or are changed ; the voice is rough and hoarse ; and more or less of a beard appears on the upper lip and occasionally on the chin.

Absence of the uterus is another organic cause of Amenorrhœa, which is equally incurable. Or if the entire organ be not wanting, the cervix may be deficient, the orifice impervious, or the vagina may be absent, imperfectly formed, unconnected with the uterus, or its sides may be adherent.

Dr Ashwell properly distinguishes between those instances of amenorrhœa in which the uterus and ovaries are absent, malformed, or diseased, and those in which these organs are present, but there is some malformation or obstruction in the vagina. In the latter class of cases the secretion takes place, but is not expelled, in consequence of the vagina being either closed by adhesion of its sides or occluded by an adventitious false membrane.

He allows that, for the treatment of cases of this order, it is impossible to lay down precise rules, and not free from peril to practise according to such rules.

“ Each case must be considered alone ; its peculiarities must be coolly reflected on ; and, while temerity is to be condemned, enterprise, short of recklessness, where the danger of non-interference is so great, is deserving of praise.

In the simpler obstructions, the operations, either by the trocar, knife, or bougie, are not difficult ; but it must not be forgotten, that such patients, with whatever facility the impediment may have been removed, and a mere incision is often sufficient, are really exposed

and either remaining within doors too much, or going out only in a carriage.

The following observations deserve the attention of the practitioner.

"The condition of the system may have been altered and improved, the treatment may have removed the plethora, but there is yet no menstruation. It may be asked, whether it be necessary in such circumstances at once to employ emmenagogues? I think not. Some months may elapse before the uterus shall perform its proper function, but eventually, menstruation will most probably occur. If, however, the health fails, and instead of a ruddy and robust, there is a pale and wan countenance, and a gradually pervading debility, the amenorrhœa will merge into chlorosis. To prevent such a termination emmenagogues may be used. But I must also observe, that if, when the plethora is removed, menstruation does not quickly occur, the continuance of the malady must not always be attributed to debility. Such an opinion leads to the premature and injurious exhibition of tonics and stimulants; for although weakness is a cause of amenorrhœa it is by no means its only condition, since often, where debility has been entirely removed, menstruation has failed to be established."—P. 65.

It appears, in short, that there is in this class of cases what Dr Parry and other physicians call *Irregular Determination of Blood*. It is probably neither the quantity nor the quality of blood that is in fault. But the circulating fluid is not carried in the proper channels. Instead of being distributed in proper proportions to the skin and extremities; the brain, the lungs, the liver, the kidneys, the ovaries and uterus; it is sent in too great quantity to one or two organs, and in too small proportion to another. In some it appears to cause violent headach and hysteria by its affection of the cerebro-spinal system. In others it causes cough and hæmoptysis by its determination to the lungs; in some it causes hæmatemesis; and in almost all, the blood which ought at each monthly period to be employed in filling the uterine vessels, and forming the catamenial secretion, is diverted and misapplied, in maintaining a constant catarrhal discharge from the vagina, or the utero-vaginal mucous membrane. The great object, therefore, in treatment ought to be, to re-establish the normal distribution of the blood, to restore that balance between the circulation in different organs which has been for the time subverted.

These views are true and applicable even to the second variety of this form of amenorrhœa, that, namely, which takes place in delicate, irritable, and hysterical females. For in these, though the state of the system be at first different, and the remote causes may also differ, yet both tend to induce, by different modes, a similar condition of the circulating system; and marks of irregular determination are not less manifest in the latter than in the former class of females.

mance of the secretion, chronic suppression, to be next treated of, will be the result.”—P. 70.

Chronic suppression is a much more tedious disease and more difficult to manage properly, and hence it may prove to be greatly more serious.

“The *pathology* of chronic suppression, where it does not depend on organic disease, may be referred to torpor or congestion in the earlier stages, and to constitutional debility in its more advanced periods. In the protracted and inveterate forms of suppression, the ovaries and uterus, in common with other organs, suffer from defective nutrition, the blood having become too impoverished to excite the organic nerves, and to supply the requisite secretion to the several tissues of the body.”—P. 72.

On the subject of Complicated Amenorrhœa the following notices are given.

“The six cases of complicated amenorrhœa were very interesting. In one, it was associated with chorea. This patient, after protracted treatment, was eventually cured by sulphate of zinc and the injection of *liq. ammoniæ* into the vagina. In another, amenorrhœa was complicated with epilepsy. The medicine prescribed was *ferri sulph., gr. i; pulv. digitalis, gr. i; pulv. myrrhæ, gr. ij; mucil. Acaciæ, q. s. fiat pilula ter die sumenda*. It is worthy of remark that these pills were persevered in for three weeks, without any injurious consequences from the use of the digitalis, a circumstance attributable, probably, to its combination with the iron. At this period, the catamenia appeared, and there has been no return of the fits. In a third case, hemiplegia was attendant on the amenorrhœa. This complication was tedious and difficult to manage. At first, the *mist. ferri c.* was prescribed; afterwards, the sulphate of zinc; and an iodine liniment was well rubbed over the spine night and morning. Menstruation was eventually established, and the patient regained the entire use of the side. In the fourth case, there was tænia with the amenorrhœa. In addition to the other remedies, the *ol. terebinth.* was curatively employed. In the fifth patient, there was vicarious discharge from the mamma, in conjunction with amenorrhœa; the *mist. ferri c.* was ordered, as well as the daily employment of the ammoniacal injection. The last patient had, in addition to the amenorrhœa, a peculiar nervous affection of one of her lower extremities, which completely subsided when the catamenial function was, by appropriate remedies, healthily established.”—Pp. 73–74.

An ample account of emmenagogues follows. These the author distinguishes into two great orders; Local or immediate Emmenagogues directly applied to the uterus and the neighbouring organs; and Constitutional Emmenagogues, or those which produce their effect through the medium of the constitution. To the first order are referred electricity, galvanism, leeches applied to the *os* and *cervix uteri*, stimulant injections for instance liquor

so far lose their full secretory power as to furnish only an increased amount of mucus. Some years ago I pointed out a similarly perverted action, occurring after labour, where aqueous discharge, occasionally in immense quantity, is poured forth instead of the lochia. And in hemorrhage from the intestines, an analogous phenomenon is sometimes presented, when a large quantity of mucus, exhaled from the villous coat, supersedes the sanguineous flow. Dewees regards vicarious leucorrhœa as a slow development of the menstrual function. Friend and Astruc were both cognizant of its occasional existence. The former denominated it 'the lymph-like menses,' and Astruc recognises it as leucorrhœa taking the place of the catamenia. Nauche, in his comprehensive, valuable work, '*Maladies propres aux Femmes*,' looks upon vicarious leucorrhœa as salutary, thus confirming the views I have guardedly expressed. He says that in 1824 he had under his care a patient, twenty-four years old, plethoric and robust, but healthy sanguineous menstruation was absent. Instead of it, there was secreted every month, and with satisfactory results as regarded the health, 'a quantity of white mucus.'—Pp. 97, 98.

IV. Of Dysmenorrhœa or Difficult Menstruation, accompanied with more or less pain and scantiness of the secretion, four forms are mentioned; the irritable or neuralgic; the plethoric; the congestive; and that from mechanical obstruction of the *os uteri*.

The first is the irritable or neuralgic Dysmenorrhœa or that in which pain is disproportionally acute, yet without indications either of inflammation or congestion in the parts. This we believe to be by far the most frequent form of the disease, and indeed to be that which constitutes the type or standard of the disorder.

The second variety is the *plethoric*, in which little deviation from the symptoms of the neuralgic form is observed; but the menstrual period is preceded by headach, flushing of the face, full quick pulse, a sense of weight in the pelvis, shiverings, and sometimes by delirium. These symptoms are followed by the catamenial discharge, sometimes profusely, with more or less coagula. In other instances which are said to be more frequent, the discharge is scanty, and consists of clots with shreds of membrane, all of which come away with extreme difficulty.

The third variety is the *congestive*, which is also attended with intense uterine pain, and a sensation as if a foreign body were confined within the uterus. There are, nevertheless, it is said, no inflammatory symptoms. In this form false membranes are expelled during the intervals of the attacks; and very uniformly an abundant leucorrhœal discharge contributes to exhaust the powers and health of the patient. It is in this variety that spuri-

of discharge, or both, without uterine bleeding; and, second, Profuse Menstruation, accompanied by direct loss of blood from the uterine arteries.

The latter, the author distinguishes into three varieties; *a*, Acute or active Menorrhagia, occurring in the plethoric and robust; *b*, Passive and chronic Menorrhagia, taking place chiefly in the delicate hysterical, and exhausted; and, *c*, Congestive Menorrhagia, generally met with at the middle or more advanced periods of life.

In the description of the symptoms attending the first variety of the second form, Dr Ashwell speaks of *spasmodic* menorrhagia in contradistinction to the inflammatory. In elucidation of the difference between the two the author observes.

“Where inflammation is present, there will be fixed pain in the uterine region; a hot dry skin, and a frequent hard and full pulse. Where spasm prevails, the pain will not be constant; but having continued a longer or shorter time, and often most severely, it will subside, and, after an interval, again recur with throes resembling the pains of labour. The discharge, too, is equally variable, ceasing for short periods, during the pain, and returning when it subsides. The pulse, during the spasm, is contracted, irritable, and quick; afterwards it becomes softer and slower, giving proof by this rapid change, of a state of system, neither of inflammation nor debility, but of irritation. The progress, duration, and severity of these attacks are extremely variable.”—Pp. 136, 137.

The distinction is important in reference to the method of treatment and the kind of remedies; and hence the following directions are given.

“In *spasmodic menorrhagia*, to which I have already referred, the pulse is irritable and quick, not hard and full. The system is not plethoric, nor the pain constant, (*vide cases*); but it subsides and again recurs. Here bleeding, nitre, and digitalis, fail to relieve; and recourse must be had to antispasmodics, anodynes, and occasionally to alteratives. It is not always easy at once to distinguish this form; but if antiphlogistic means have been tried unsuccessfully, the patient will often be cured by remedies of a different class. Dr Gooch says, ‘that a lady labouring under spasmodic menorrhagia went through the whole routine of antiphlogistic treatment without any benefit. I then gave her, he adds, one grain of ipecacuanha every hour; in eight hours she became nauseated and sick, and the discharge immediately ceased. This state of nausea was kept up for a day or two, and the discharge did not recur. When you have a case of menorrhagia, attended with a quick and irritable pulse, the pain subsiding and recurring, you may be certain that it arises from spasm or irritation, and that it will be relieved by antispasmodic remedies. The two best are ipecacuanha taken into the stomach, and assafoetida, with opium injected into the rectum. A grain of ipecacuanha is to be taken every hour till nausea is produced; which state must be maintained for a day or two, by repeating the same

All the forms of this disease, and especially that which is chronic, are perplexing to treat. All means that promise to stop the discharge may have been tried; and too often for a time, all are unavailing and inefficient. Astringent injections, sometimes vegetable, sometimes metallic, are the means which have been most commonly employed. Among the latter, nitrate of silver in solution has in general been found most successful. But it is well remarked by the author, that any one of the ordinary metallic or vegetable astringents in lotion or in injection, however good they may be, soon lose their effect. Hence, may be deduced the inference that it is best to vary the lotion or injection.

On the other hand, it should not be forgotten that, in attempting the cure of Leucorrhœa, attention to the state of the general health, and especially to the condition of the cutaneous and mucous surfaces, with attempts to improve them, are of indispensable importance. The due regulation of the functions of the alimentary canal by means of laxative and aperient medicine, cleansing the surface, and rendering uniform the circulation of the skin, by means first of the warm bath, then the tepid bath, and finally, the cold bath, cautiously administered, will tend more to remove the disease, and to keep it away, than almost all local measures however active and powerful.

Turpentine and cantharides, which act on the mucous surfaces, Dr Ashwell states he has given often advantageously and a good many times with curative effect. This is in imitation of the practice of Dr Dewees, who further begins the treatment by placing the patient on milk and vegetable diet, and, in some instances, causing her to lose blood.

For a class of cases, which we believe were quite similar, Dr Gooch had a practice of administering small doses, that is from one to two grains of ipecacuanha two or three times daily; while, at the same time, he recommended the constant use of flannel trousers, and careful regulation of the diet. This practice, we have understood, was remarkably successful. It matters little whether ipecacuanha or tartrate of antimony be given at first, for either might do, in order to rectify the circulation of the intestinal mucous membrane, and abate the chronic inflammatory state of the utero-vaginal mucous membrane. One objection to the use of ipecacuanha is, that it tends rather to constipate the bowels than to relax them. But this may be obviated by combining with it a little rhubarb, aloes or scammony. It is indeed of paramount importance to cause the colon and rectum to be duly and regularly emptied, by whatever means this is accomplished. Dr Ashwell himself states, that he lately saw a case in which the discharge, after being for weeks excessive, was restrained by giving five or six doses of blue pill, followed by an aperient of salts and senna. The first motions were highly offensive and

taken gin-twist or brandy and water, get well of these complaints; the recovery has been effected not in consequence of the use of these articles, but in spite of them, and in opposition to their use; and the patients most certainly would have become better in shorter time without them. Such, however, is the effect of the vulgar error, which assigns to these liquors the name and character of support, and the power of increasing strength; such is the craving among invalids for them; and so deficient are physicians in fortitude, that while the former entreat to be allowed some of these articles, because they are so habitually taken, the latter have not the firmness and sense to refuse them, and the candour to explain to their patients that the latter are prolonging their sufferings, and increasing, rather than diminishing, the weakness of which they complain. Let this be altogether reformed.

The tenth chapter contains an elaborate and correct exposition of Hysteria. But our limits permit us not to enter upon the consideration of a subject so extensive and so complicated. We recommend the chapter to the attention of readers.

In the eleventh chapter is given an instructive account of that perplexing disorder, Irritable Uterus.

This disease, which consists in a permanent and painful sensibility of the uterus, especially its neck, Dr Ashwell shortly characterises as a constant Dysmenorrhœa. The disease usually takes place during the middle period of life, and is exceedingly difficult to cure, or even to mitigate in its symptoms. In general it is a disease of the married, though he allows, that single females after thirty, or at the approach of the period of catamenial decline, and widows, may be its subjects.

The description of the state of the parts deserves attention.

"In this state the uterus is almost always a little lower in the pelvic cavity than natural, and by some authors it is said to be even constantly prolapsed. An external examination of the lower part of the abdomen gives pain; and if the finger be pressed behind the pubis, and round the pelvic brim, a paroxysm is not unfrequently induced. But it is by an examination of the neck of the uterus that we discover the real nature of the malady. It cannot be said that the body of the organ is free from morbid tenderness; but the sensibility of the cervix is often so exquisite, that the patient shrieks, and is thrown almost into a fit of hysteria if it be rudely touched; and on several occasions, by patients at Guy's, I have been requested to forego any renewed examination, because so many hours elapsed before the intense suffering subsided, even after the slightest pressure. Dr Gooch believed that the tenderness was confined to the uterus, and that the finger might be pressed against the sides of the vagina without causing uneasiness. My experience has not confirmed this view, having frequently observed a more than ordinary degree of sensibility and heat in this canal. In acute in-

inflammation of the cervix uteri, a much less common disease, the observation is, I think, perfectly true; as then pressure on the vagina, immediately contiguous to the neck, or on the body of the uterus, is borne without any suffering at all. In irritable uterus, the cervix is often somewhat shortened and expanded, and occasionally puffy and swollen, and the lips of the are more than naturally closed.

“Dewees especially remarks, that in all his cases a pulsating, throbbing, or fluttering sensation within the vagina or in some part of the pelvic cavity, always disagreeable, though not constantly present, and sometimes interrupting sleep, frequently occurred. Such symptoms have not existed in marked degree in the examples I have seen; but in several the throes of the uterus have been painfully severe.”—Pp. 241, 242.

The facts now stated, would tend to create doubt, whether all this pain and suffering consisted merely in elevation of sensibility, without some change in organization if not change in structure. The symptoms now enumerated, would lead to the conclusion, that the vascular system of the neck at least of the womb was overloaded, the vessels distended, the blood prevented from moving through them; in short, that there was chronic congestion of the vessels of the cervix and vagina also, perhaps even of the body of the uterus. Accordingly, Dr Ashwell subsequently observes:—

“That the disease in question should be regarded as a modified inflammation of the cervix uteri, is a view, in accordance not only with symptoms, but with the results of the most successful treatment. It is difficult to understand, that there shall be redness, which I have several times seen by the speculum, heat, permanent pain, and tenderness of the neck of the uterus, a glandular part, without believing that its vascular and nervous structures, shall have undergone some change. Judging also from the marked relief afforded by cupping, leeching, aperients and spare diet, in Dr Gooch’s own cases, what more tenable and satisfactory conclusion can be arrived at, than that the so-called irritable uterus is really dependent on subacute or chronic inflammation; a position, the truth of which is fully substantiated by those changes of structure, which, although slowly, and not till after many years, have nevertheless occurred, in cases, which till then, were regarded and treated as examples of irritable or neuralgic disease.”—Pp. 243.

The whole of the subsequent observations are valuable, not only for the facts which they adduce, but for their tendency, and the inferences which they unavoidably and unambiguously suggest. We can, however, admit only the following as deserving attention in illustrating the true nature of the disease:—

“In a poor woman of the name of Turton, long under my care at Guy’s Hospital, I had an excellent opportunity to test the truth of these opinions. She was an out-patient for three or four years, and during the whole period was suffering from irritable uterus. Constantly I pointed out her case to the pupils, as an excellent example

of this affection. Numerous remedies were tried; and sometimes relief was obtained, but more frequently the various measures independently of narcotics, did little or no good. At length, after nearly six years, induration occurred. She became an in-patient, and the entry by the clinical clerk in September 1838, is as follows:— 'The cervix is extensively destroyed by ulceration, and is also indurated.'—Pp 244.

Assuredly, it would not be legitimate reasoning to deduce conclusions from one case, or even from several. But when it is very constantly found that this irritable state of the cervix is connected with heat, swelling, some delapsus as it were, and that it is relieved, as we have several times seen,—by no means so effectually as by local depletion by means of leeches; it amounts as near as may be expected in pathological reasonings to proof, that, though perhaps there may be exalted innervation, yet this depends upon exalted vascular orgasm, upon congestion of the parts, and great derangement in their circulation.

In the treatment of this disorder, Dr Ashwell recommends that two indications must be observed, namely, to mitigate local suffering and to sustain and improve the general health.

At first he advises absolute repose in the horizontal position, not for weeks only, but for several, perhaps many months. The patient, however, should be carried into the open air, and if possible be placed on the sea coast. When exercise is attempted, it should be in the recumbent posture. The diet should be good, not stimulating; milk in every form in which it can be procured. Among drinks he allows claret and bitter beer. On the other hand, confinement within doors, low diet and aperients must be avoided.

As to local treatment, scarifications of the *os uteri* seem to be the only means of which he approves. Even much bleeding by leeches he regards as hurtful and to be avoided. Of the use of pessaries, he speaks favourably. But they are articles which, we think, should not be used, unless it be found that it is impossible to do without them.

We have here completed the account of the First Part of the work of Dr Ashwell; and we trust that the sketch will enable readers to form their own opinion of its merits. We have had such frequent occasion to express an opinion in favourable terms, that it becomes unnecessary to add any thing upon that head. But it is right to inform readers, that they will find in this volume a large amount of excellent information, the value of which is in no small degree enhanced by the cases with which each subject is illustrated.

We defer to another season the examination of the Second Part of the Work. Meanwhile, we can assure readers that its merits are in no respect inferior to those of the First Part. The whole work is in short a Practical Treatise of great value.

ART. II.—1. *Practical Observations on Mineral Waters and Baths; with Notices of some Continental Climates, and a Reprint, the third of the Cold Water Cure.* By EDWIN LEE, Esq., Fellow of the Royal Medico-Chirurgical Society, &c. &c. London, 1846. Post 8vo. Pp. 134 and 42.

2. *Bradshaw's Companion to the Continent. A Descriptive Handbook to the Chief Places of Resort; their characteristic Features, Climate, Scenery, and Remedial Resources; with Observations on the Influence of Climate and Travelling.* By EDWIN LEE, Honorary and Corresponding Member of the Principal Continental Medical Academies and Societies. London, 1851. Folscap 8vo. Pp. 408.

3. *Lectures on the German Mineral Waters, and on their Rational Employment for the cure of certain Chronic Diseases.* By SIGISMUND SUTRO, M.D., Senior Physician to the German Hospital, Corresponding Member of the Physico-Medical Society of Erlangen, &c. London, 1851. 12mo. Pp. 431.

The mineral waters of the German States have long enjoyed, among the natives of those countries, considerable celebrity for the alleviation and removal of certain morbid conditions of the human body; and the physicians of the different countries of Germany have for a long series of years reposed considerable, it may be said, in some instances, a high degree of confidence, in the therapeutic powers of the different mineral springs in that extensive and varied country.

This confidence, however, has not in the same degree been shared by physicians in other countries. These gentlemen have been in all instances slow to admit, and in some instances have not admitted in the unqualified manner believed by the physicians of the German states, the therapeutic powers of the German mineral waters in the cure of disease. This disinclination was not wonderful, considering the circumstances under which the therapeutic employment of the German mineral waters became known.

During the continental wars of the first Napoleon, little was known of these numerous sources beyond the boundaries of Germany itself. To the physicians of Prussia, the Rhenish provinces, Saxony, Bohemia, and Austria, their effects and uses were in a certain degree known, as means of treating various slight chronic disorders, the result of indigestion and its effects. But

tains are generally allowed by geologists to be of volcanic origin, containing trachyte, basalt, and similar allied rocks. Now in the former arise the springs of Berthrich on the west of the Moselle; and in the springs of Ems on the Lahn, and Wiesbaden on the east of the Rhine, who ever examines with a geological eye the Heyrich or Taunus range, and considers the site [of Wiesbaden, will see that the latter district forms as it were the centre of a crater, and that the hot springs are probably the outlets of long chimneys, arising from a considerable depth in the interior of the globe.

Dr Sutro represents sulphurous and chalybeate springs as mostly independent of the general character of a region, and as probably isolated products of local causes; whereas acidulous and thermal springs can in general be traced to certain characteristic mountain chains. Thermal springs also, as a general principle, are supposed to proceed from deep lying strata; whereas cold springs issue in general from those which are superficial. Of the former, the Wiesbaden springs are good examples; whereas the neighbouring cold springs of Geilnau, Fachingen, and Selters, proceeding from the upper lime strata, containing slate, are believed to exemplify the latter proposition.

Saline springs are supposed to derive their origin from strata of rock salt, through which they pass, and which belong also to late formations, being frequently surrounded by lime, gypsum, and clay. In some instances it is not improbable that these are outlets of considerable subterannean brine—salt springs, rivers, or lakes; but as these must be supposed to derive their saline impregnation from beds of rock salt, it refers itself to the same general principle.

The saline springs of the German states may in general be traced in the direction of the great deposits of salts. The rich salt mines of Austria supply the brine springs of Ischl, Reichenhall, Rosenheim, on the west. The salt deposits near the banks of the Rhine, from Basel to Aix-la-Chapelle, supply the saline springs of Baden-Baden, Kreuznach, Wiesbaden, Soden, Nauheim, Schwalheim, Salzhausen, Burtscheide, &c.

In lecture third Dr Sutro considers the general chemical constitution of mineral waters as agents acting on the animal economy. The characters, which constitute and distinguish mineral waters, he observes are their peculiar composition and physical properties of taste, colour, temperature, and specific gravity; and above all their specific effects on the human organism. Water absolutely pure and free from saline impregnation, seems to be rare. The water of several wells used for drinking in Berlin, Hanau, and other towns contain from five to ten grains of solid ingredients dissolved in every sixteen ounces, yet without being in consequence

fact, when a curative reaction is to be expected, or when this general external derivation is to be safely employed, to restore diminished cutaneous innervation. But they must be considered as *extremes* in extreme states; at least for the whole body; for in such general torpor some internal organ is mostly in a state of irritation, forbidding the employment of such a violent remedy; but in the shape of partial baths, they may often be employed as excellent local derivatives.

"*Hot mineral waters* create a somewhat less sensation of heat, because part of the caloric is required to keep the dissolved ingredients in a liquid state. This is particularly the case in mud-baths, in the so-called *chalico* (earthy) and *theia* (sulphurous) *thermæ*; less so in the alkaline or saline *thermæ*. In the latter, the irritation of heat is supplanted by the chemical stimulus of soda or chlorine, corresponding with the other stimulus at least in its secondary result.

"*Baths of a temperature* between 20° and 30° Reaumur (77° and 100° Fahrenheit) would not produce any influence on the temperature of the organism if the animal heat had not the function of furnishing a regulating excess to the surrounding media. In some individuals this excess is very small, and would be withdrawn by a bath of 28° Reaumur. In others, again, of more robust and plethoric habit, this heat would produce effects described as *appertaining* to baths of 30° and 36° Reaumur.

In the lukewarm and cool bath, the warmth acts no longer as a stimulus inherent to the water. But the absorption of internal heat must influence those organs which perform the function of calefaction. The amount of heat which each atom of water requires to put itself into equilibrium with the heat of the body, and the rising of the warmed layers from the surface of the body upwards must materially lower the amount of animal heat, and primarily depress the external innervation; but the consequent reaction induces a powerful impulse from the interior to the circumference, tending to an increased production of heat, and a heightened metamorphosis on the surface.

"If the individual leaves a cold bath, the reaction produces such an excess of warmth as to guarantee a certain stability of external innervation and resistance to the changing temperature of the air (unless it be violently agitated). The air itself being a bad conductor, and requiring little latent heat for maintaining its gaseous form, does not deprive us of a great amount of warmth as already shown. The epidermic redness produced by a cold bath certainly resembles the visible effect of a hot one. But whilst the consequence of a hot bath is reaction to the interior, redness with congestive fulness of the epidermis, being the commencement of the phenomena, in a cold bath, external redness and increased circulation are the closing phenomena resulting from a reaction issuing out of the centre towards the exterior. The warm bath acts as a sudden excitant of functions, the cold as a sudden retarding influence. The individual

organic resistance will decide whether the reaction will have a local or general character.

“ Suppose, for instance, the urinary organs to be in an irritated condition, a sudden immersion into a cold bath, which momentarily prevents perspiration, will produce increased urinary secretion, or spasms of the secreting organs. If mucous membranes are abnormally affected, over-stimulation and great irritation may ensue through the sudden action of cold. The functions of the muscles sometimes become insensible and immovable through this diminished innervation.

“ When increased calefaction takes place by some irritation of the peripheric nervous system, a cold bath will absorb this excess of heat, and violent reaction need not be apprehended, from the energy of the nervous system having been previously weakened through the over-stimulation. The cold bath will only be beneficial when its effects assume the type of fever—cold, warmth, increased secretions, &c.

“ The duration of the cold bath is of great importance. After the first effects of weakened innervation have been counteracted by reaction, the steady-heat absorbing-power begins to act. The general secondary reaction, beginning with rigors, will appear, sooner or later, according to the individual power of resistance.

“ Smaller quantities of water rapidly exchange their temperature for that of the blood. The evaporation consequent on the liquid applied always binds a certain amount of heat. Moist bodies under thick covering stimulate the cutaneous system to a greatly increased serous secretion. They act like a local vapour-bath, with this modification, that the body itself supplies the necessary warmth. Such moist applications and fomentations may be reckoned as powerful sudorifics, and might be very beneficially employed with sea or saline water in complaints characterised or produced by suppressed or retained perspiration, and by heightened nervous susceptibility to external influences.

“ *Vapours* can be employed in much higher temperature than water, being worse conductors, and possessing a smaller capacity for caloric. But you may ask, ‘ Why should they be better borne than air?’ The answer is very simple; because they require a great amount of heat to be kept in the gaseous form, which is not the case with the permanent gaseous mixture composing our atmosphere. They also less impede evaporation, in fact, promoting it. They are very useful where the liquefying process is to be heightened, where perspiration has been retarded, where noxious matters are to be excreted by the skin.”—Pp. 47–50.

Besides the distinctions above mentioned, Vetter divides all mineral waters, principally in reference to their action on the human body, into two large classes; the *Akratopegæ* [*α, χαλρος, πηγή*] or those springs which have constituents, as it were, powerless, though in efficacy they may be considerable; and *Synkratopegæ*, [*συν χαλρος πηγή*] or those mineral waters in which the power of the constituents bears a certain proportion to the observed effect.

The waters belonging to the former class are also called *chemically indifferent*. They are clear, tasteless, generally inodorous. In a few instances they inhale a feeble odour of sulphuretted hydrogen. Their specific gravity is nearly the same as that of water, and they contain in sixteen ounces less than five grains of solid ingredients, and not above the tenth part of the quantity of gas, which might correspond to the degree of their tension. Their constituents are such as not to exhibit strong effect in small quantities.

These chemically indifferent springs or *Akratopegae* are again subdivided into two orders.

The first, the *Akratothermal*, or chemically pure thermal springs; as Gastein, Landech, Pfeffers, Teplitz, Warmbrunn, Wildbad, &c.; and

The second, the *Akratokrenae* or chemically pure cold springs.

The *Akratopegae* arise in general out of primitive mountains, or those which are composed of fossils, little soluble. They are alkaline or earthy, deficient in gas, with the exception of nitrogen and sulphuretted hydrogen. To this class of waters the author ascribes several properties, which are either not easily understood in this country, or not very easily believed. They are said to stimulate more than common baths of the same temperature, in consequence of their possessing a greater degree of latent heat. They are said to possess a highly penetrative power. The power of *solving animal evaporation* is to us an expression unintelligible.

Of the chemically indifferent thermal springs, Wildbad in the Black Forest, in latitude 49° , longitude 8° , is the first to which the author directs attention. It is a savage wild place, situate, about 1300 feet above the level of the sea, in a valley, around which the mountains rise to another 1500 feet. The surrounding mountains consist of ferruginous red sandstone and granite. The springs flow out of the clefts of granite rocks, gray in colour, granular in character, in four divisions from north to south, forming several independent basins. The springs vary in temperature from $25\frac{1}{2}^{\circ}$ R. = 88° Fahrenheit to 30° R. = 99° Fahrenheit. The waters contain chiefly chloride of sodium, carbonate of soda and lime, with some sulphate of soda, with iron, manganese and potassium in very small quantity, and a large proportion of nitrogen.

It can scarcely be the chemical ingredients which produce the beneficial effects resulting from the use of the Wildbad springs, though the author ascribes part of the utility to the chloride of sodium which they contain. This explanation, nevertheless, is scarcely intelligible, as the quantity of this salt is very small.

The following explanation may therefore be more entitled to attention.

“ If we see the use of a mineral water, causing distinct retrogression of these anti-vital phenomena; if we perceive gouty concretions

had no communication with the duodenum, no trace of gall-bladder, and in no particular resembled the liver. The liver and spleen were wanting; the diaphragm cleft, a narrow strip at each side being barely cognizable. The lungs were wanting. The trachea terminated about the place of the first dorsal vertebra, in a knob of cellular and parenchymatous substance not larger than a hickory nut. At the left of the *spine and higher in the thorax than usual*, was seen the heart without a pericardium. It was placed very obliquely as well as high up, so that its apex could not have struck below the third rib; it acted with surprising force and regularity between 60 and 70 times in a minute, and appeared to carry on the circulation, as the aorta pulsated very distinctly. His attention was immediately rivetted on this unexpected phenomenon—as he always wished to see things as they appear to the senses, and understand them according to the dictates of reason.

The words of the author must give the subsequent part.

I have been long puzzled on the subject of the circulation, by the discrepancies of facts and theories exhibited by systematic writers. Whenever I have found it difficult to adapt plain phenomena to preconceived notions, instead of straining the facts to fit the notions, I have discarded the notions and retained the facts. In the case before us, I shall endeavour to describe with scrupulous exactness, the action of the heart exhibited to my own sight and touch, and I think I can show that the same phenomena have been seen and felt by others, and rejected as incompatible with some favourite theory.

Viewing the heart and feeling it during the pause, although it was perhaps less hard than in either diastole or systole, yet it did not appear in that state of complete relaxation which physiologists ascribe to it; perhaps the antagonizing powers were merely in equilibrio; possibly the contracting power predominated. From this state of quiescence it would suddenly spring, dilated with surprising force, its apex elevated with a jerk, finish its double action in the twinkling of an eye, and fall back as suddenly to the stillness of death. No motion like sudden or gradual relaxation appeared in its fibres, though vigilantly watched for, and indeed expected; the diastole, which always preceded, appeared to commence in the venous sinus, and pass without interruption and with immense velocity to the apex; the systole pursued the same course with equal velocity.

When it is considered that this double action was completed in a space of time less than half a second, perhaps not more than a third, for the time of the pause appeared to be nearly, if not quite double the time of the action, you will readily comprehend the difficulty of noting particulars in their exact order; that the action passed from base to apex was visible enough—also that the diastole preceded the systole; but so rapidly did the systole succeed the diastole, that I could hardly ascertain with perfect satisfaction to myself, whether the diastole of the ventricle was *entirely* completed before the systole of the auricle commenced. My decided impression however is, that at one and the same moment the diastole is finished, and the systole commenced.

It may here be asked, if the diastole continues for so brief a moment, is there sufficient time left for the blood to occupy the cavities? I would answer, were dilatation a passive movement, it would certainly require more time for the pressure of the blood to overcome the *vis inertiae* of the heart, and dilate its cavities, but when we consider that the blood is accumulating in the adjacent vessels during the pause, and under strong pressure, and when we estimate the great force and velocity with which the cavities dilate and the consequently powerful suction they exert, we can hardly doubt that they are *instantly* filled; at the moment of di-

astole the apex of the heart was elevated ; at the moment of systole it fell back with great suddenness.

After watching the heart in situ fifteen or twenty minutes, and frequently grasping it to ascertain its force in dilating, I separated it from its connexions ; its action continued without sensible diminution, either in frequency or force, and in that state it was put into the hands of different individuals, that they might be qualified to testify the fact.

I then proceeded to examine the state of the valves and septum, which were found as usual ; the *foramen ovale* open, the pulmonary vein and artery, cava and aorta of the proper size. In this state, with auricles and ventricles laid open and roughly handled, the heart was thrown into a basin of cold water. After examining the other viscera for some time, I returned to the heart, and was surprised to find it still moving, feebly it is true, but with perfect regularity ; the attention of the spectators was again directed to this unexpected state, and it was again placed on the hand of Mrs F., who attended the accouchement. At this period I felt myself sinking so fast, that I was reluctantly obliged to retire without inspecting the viscera of the pelvis, tracing the lesser circle of circulation, or even injecting the funis, the course of which I could not even conjecture without such aid.

In a few brief propositions I will here recapitulate the inferences which this interesting case has fully established in my mind.

1st. Systole and diastole are the natural muscular actions of the heart.

2d. The force of the diastole is equal to, if not greater than the force of the systole.

3d. There is no pause between auricle and ventricle, either in diastole or systole, the action passing from base to apex with great velocity ; *perhaps* accelerated, *certainly* not retarded.

4th. There is not only no pause between the diastole and systole, but the latter almost seems to appear before the former vanishes, thus presenting the appearance of one compound action in which all the parts concerned cooperate perfectly.

5th. The diastole always precedes the systole in the natural order of action. BICHAT's experiments led him to the same conclusion.

6th. Similar actions in similar parts of the different sides are always synchronous.

7th. The time of action, including systole and diastole, is less than the time of the pause.

8th. The pause succeeds the systole of the organ.

9th. The impulse of the apex against the side of the thorax is caused by the diastole of the auricles, especially the left—for as the distance of the axis of the heart from the diaphragm must be increased by the increase of its transverse diameter in diastole, and as it is firmly tied down at its base, this increase of distance of the axis can be effected only by its turning on the distended auricles—as each action of the heart is performed by a convulsive jerk, the force of the impulse is naturally accounted for.

10th. There is in the heart a *vis insita* or *vita propria* ; this is demonstrated by its perseverance in action so long after its entire separation from all influence of brain, nerves, and ganglions.

The system of action deduced from this case, exhibiting a perfect cooperation of the whole organ in each action and concurrence in repose, seems much more simple and consonant to nature than the complicated series of alternations so zealously inculcated by the ancients, and which, with but very little modification, has kept possession of the schools to the present day. Passive diastole of auricles—active systole of auricles—passive diastole of ventricles—active systole of ventricles. In this se-

ries we are presented with a perpetual union of action and inertia in the same organ—one-half being always asleep, and part of the power of the waking half continually wasted in overcoming the inertia of the sleeping half. This I never could receive; but after witnessing the simple series of nature, diastole of the whole organ, systole of the whole organ, repose of the whole organ, the former arrangement appears only as a clumsy, ridiculous contrivance.

Most of the errors connected with the subject of the circulation, from HARVEY to the latest writers, may be traced to one and the same source, that unaccountable delusion, that during its diastole the heart must be in a perfect state of inertia. "Fibris enim propriis dilatari repugnat oculo." (HALLER, prim. lin.) "Neque relaxatio cordis aliquarum fibrarum actio naturalis est." (Coroll. de cord. mot.) "Diastolen nullius strati musculosi opus esse, sed relaxationem et meram inertiam." (De sang. mot. ejusq. causis.) Consequently the inert auricles must be dilated by the pressure of the blood from the contiguous veins, and consequently power lost; they must be stimulated to contraction by the blood forced into their cavities; the whole of the force with which they contract must be wasted in dilating the torpid ventricles, which in their turn must be stimulated to contraction by the fluid forced in, and by no other cause—"Non aliam causam esse hujus in motu pertinacis præter ipsum perpetuum sanguinis adfluxum." (De cord. mot. a stimul. nato.) Thus, from a single preconceived error, a series of absurd alternations have been ascribed to the heart. "Ut auricularum contractio cum ventriculorum relaxatione, ventriculorum systole cum auricularum diastole conjuncta sit." (De mot. sang. per cor., § xi.) A system repugnant to nature, to reason, and to the recorded observations of its advocates—a system by which the heart is deprived of about three-fourths of its power as an agent in circulation; for, by denying the power of the diastole, you deny at least half the power of the heart; and as the whole systole of the auricles is wasted on the diastole of the ventricles, that, too, must be deducted, leaving only the systole of the ventricles.

Such is the system of Harvey and his successors:—"Hæc in cordis motibus series Harveio quidem placuit omnibusque anatomicis ab eo qui viva animalia aperuissent." (De mot. sang. per cor., § xi.) Although so many alternations successively performed in *one second*, would seem to leave little time for pause, yet I believe all physiologists, ancient and modern, concurred in the belief of at least an apparent pause, after the systole of the ventricles.

"Expulso sanguine cor quiescit ex solo stimuli defectu" has been generally received as an axiom till the time of LAENNEC. He supposes, that the heart pauses while the ventricles are subjected to the double stimulus of distension and of the blood; this error, for error it is, grows naturally out of the theory under review. Laennec, in exploring the thorax, hears two sounds in rapid succession, followed by an interval of silence, marking, as he supposes, the pause of the heart; the first sound is accompanied by an impulse felt in every part of the arterial system; this impulse is probably the cause—it cannot proceed from the systole of the auricles whose impulse cannot reach the arterial system, it must therefore be ascribed to the systole of the ventricles; the second sound must have some active cause; diastole is passive therefore silent; the only remaining cause then, to which it can be ascribed, is the systole of the auricles. This course of reasoning seems to lead more directly than any other to Laennec's conclusion, which after all is a *non sequitur*; for I more than suspect, that the true cause has been omitted in the enumeration; besides the premises are all false. MAGENDIE admits that the heart is not entirely passive in diastole; BICHAT affirms that it is power-

fully active, and the case before us confirms the fact—the diastole then is as likely to be heard as the systole.

That both systole and diastole of the ventricles are interrupted continuations of similar actions of the auricles, seems also to be established; their sounds, if any, must therefore be indistinguishably blended.

If the auricle and ventricle co-operate in the diastole, as I think I have seen them do, there is then a continued column of blood from the venous to the arterial valves; consequently, the first impulse of systole at the base of the auricle is felt through the whole arterial system. Again, considering the diastole of the auricle as the cause of the impulse of the apex against the side, (Prop. 9th,) and remembering also the celerity with which the systole follows it, (Prop. 4th,) I think the impulse of the apex against the side, and the commencement of the systole in the base, must be so nearly synchronous that no variation can be distinguished by the senses—the vibration generally perceptible in a strong pulse, may be ascribed to the increased impetuous from the superior power of the ventricle. Haller, after ascribing the stroke against the side of the systole, and especially to the curve given by that action to the apex, is obliged in candour to acknowledge that there is a further cause, to wit, the diastole of the left venous sinus—"Antrorsum præterea pulsus a sinu sinistro venoso, qui eo imprimis tempore repletur." (Prim. lin. ciii.)

If this view of the circulation be correct, and I at least must believe that it is, we ought to proceed without delay to review our system of diagnosis in cardiac diseases, more especially that part which depends on the stethoscope.

That Haller and many of his predecessors, contemporaries and successors, who practised vivisections, saw the very same actions of the heart which I have described, but explained them to suit preconceived notions, might be proved by numerous quotations. They saw and felt the heart dilate with force. "Observavimus aperto pectore et pericardio, &c. &c."—"prementem digitum valde repellere." (Stenon. act.) "Cordum pulsat, digitum repellit non sine robore." (Haller passim. de mot. cord.) Can any one believe that the finger was repelled by such a contraction—"ut basis apici, ventriculus ventriculo fiat proprius"—the organ literally shrinking in all its dimensions would have receded from the hand; would not forcible dilatation have been a more plausible agent of strong repulsion? but the heart is passive in dilatation, therefore the finger was repelled by contraction. They saw the heart dilate and contract from the energy of its own innate power, without mechanical distension or stimulus, "Motus cordis non turbatur capite resecto neque corde avulso." (De mot. cord.) Hundreds of similar observations on this point might be collected from various authors. They saw the same blended action of diastole and systole, as I have described, as even Harvey acknowledges. (Exerc. de motu sang.)—"In calidioribus animalibus motum cordis et auricularum videri quale simul fieri." Haller expresses himself to the same effect in the following passage:—"Dum alacriter auriculæ micabant intervallum quo earum motus præcedit cordis motum diu ægre distincti donec animal vegetum fuit."* (De mot. cord.) Again—"Et tamen candoris mei est, phenomenon non dissimulare quod ipse vidi et ante me Lancisius, omnino nempe credas te videre, eodem tempore, et duas auriculas constringi et duos ventriculos."† (De mot. cord.) Haller in this passage acknowledges that the facts he states are inconsistent with his theory; on this ground LANCISI arraigns the series of Harvey, and proposes one perhaps nearer the truth, but still defective.

A strong argument in favour of the series of actions suggested by our case, may be founded on the structure of the organ itself. Who ever

* Exper. 319.

† Exper. 394.

inspects with candour and attention the structure of the valves, will find it difficult to persuade himself that they are adequate to the function generally assigned to them ; he may easily ascertain that they offer an *impediment* to the reflux of the blood, but hardly that they afford a complete obstruction ; but as the contraction of the auricles is less powerful than that of the ventricles, such reinforcement seems necessary to prevent reflux.

Of this imperfection of the valves Haller was convinced by the inspection of living animals, and remarks, " Non adeo fidelem valvularum custodiam esse." (De mot. sang. ejusq. caus.) Again he says, " Neque adeo in vivo animali, tantam esse valvularum efficaciam ut vulgo creditur." If such is the inefficacy of the valves, suppose the auricle in diastole and ventricle in systole, (according to the received opinion), the reflux must seriously diminish the quantity of blood sent to the arterial system. But suppose with me, that the auricle is in a state of contraction, the only safe-guard against reflux at this time, then will the whole of the blood contained in the ventricle be transmitted by its systole to the arteries ; the auricle being less firmly guarded in this way, we perceive a reflux at every contraction, which however is checked by the column advancing in the viens ; this simple explanation of the reflux appears less objectionable than that of Magendie.

While Bichat affirms that the heart dilates with a force which no effort of the hand can prevent, is it not surprising that he has neglected to apply a power so great and so obvious to the elucidation of that obscurity of the venous circulation, on which he acknowledges authors have hitherto shed few rays of light ; to you it is unnecessary to demonstrate that the pressure of the atmosphere on the veins external to the cavities must be propagated to the termination ; that the cavities being always full, there is a continued pressure on the vessels within them ; add to this the powerful dilatation of the heart, auricles and ventricles cooperating in the action, as I have seen them, and you have all the requisites of a powerful sucking pump operating perpetually on the venous system,

That the heavy mass of blood contained in the inferior part of the trunk and lower extremities could be elevated to the heart of a tall man standing erect, by the sole force of insensible contraction of the capillaries, (as Bichat supposes), is so startling an hypothesis, that were I obliged to say *credo* to it, I should certainly add in the words of the old churchman, "*quia impossibile est.*" Besides, this insensible contraction of the capillaries has not been demonstrated ; the pump I think has been, and that its agency in the venous circulation is more powerful than that of any other cause yet assigned, or than the combined powers of all the other causes, I have no doubt ; other causes not immediately connected with our case I shall pass by with one exception.

Dr BARRY has attempted to show that the venous circulation is greatly promoted by inspiration. The blood he thinks rushes through the veins towards the cavity of the thorax, to fill the vacuum occasioned by the elevation of the ribs ; he forgets that the air also rushes towards the opening vacuum, and that if two fluids, one of which is highly subtle, the other dense and viscid, present themselves under the pressure of the atmosphere at equal apertures, to an opening cavity, the quantities of these respective fluids, entering in a given time will be found in the increase ratio of their density and viscosity. If Dr B. has any doubts of this truth, as he is fond of experiments, let him procure a large syringe furnished with two pipes of equal diameter, one being curved ; let him immerse the aperture of one in fluid blood, or any other fluid equally dense and viscid, the other remaining in the air—then draw back the piston and ascertain the ratio of air to the other fluid in the machine,

This case is regarded by the author as fully confirming the explanation which he has given of the movements of the heart.

The reporter in the *Archives* remarks on this case, that it serves to show the facility with which some observers see proofs in favour of a previously received opinion. As the sternal opening, he remarks, was not complete, but was shut up by means of a membrane, it was impossible that the heart could be watched with sufficient precision in its movements. Neither could the heart be seen, nor held by the hand, so as to appreciate in an exact manner its times of contraction or of dilatation. Even though this impediment did not act in other cases of *Ektopia*, such as that of M. Follin, yet in most of these cases, the observations are made with so great difficulty and so little exactness, that they are necessarily imperfect and unsatisfactory, and cannot justly be applied to sustain any given theory. The observation is certainly well founded, that often the opinion of the observer is substituted for observed facts.

He allows, nevertheless, that this objection does not apply to the case of Dr Robinson, in which the heart, being completely exposed, it was possible to follow its movements in all directions.

It is further added, that in two cases of *Ektopia* as well marked as that given by Dr Robinson recently presented to the academy, and of which that of M. Follin is manifestly one, it was possible to demonstrate the fact, that the heart went from its state of repose by a sudden enlargement of all the diameter of the ventricular portion, with elevation of the apex, and that at this instant, these movements constituted not the *systole* as is believed, but the *diastole*. The *systole* terminated rapidly the *diastole*, and its character was the retraction of the apex, and the return of the dilated ventricle to the normal state. This order in the movements of the heart was observed by several physicians present, who witnessed the fact.

It will be important, therefore, if opportunity of observing similar cases shall present itself, to be known what it is requisite to ascertain, to determine the nature of the ventricular movement which follows the state of repose. Is it constituted by a sudden enlargement of all the diameters, exactly like the arterial pulse, and the movement which is usually regarded as the *systole* is not, on the contrary, the *diastole*, as Dr Robinson affirms, and as appears to have been seen in the cases presented to the Academy of Medicine?

Note.—It may be proper to mention that most of these passages in Latin quoted by Dr Robinson are from one or other of three Essays of Haller, *de Motu Sanguinis per Cor.* the Experimental Essay *de Cordis Motu a Stimulo Nato*, and the Experimental Essay *de Cordis Motu*. OPERA MINORA, Tomo ii. Lausannae, 1763. 4to.

These passages are so erroneously printed in the work containing the Essay of Dr Robinson that they are altogether unintelligible.

2. *On the PAROVARIIUM in the Female; the organ analogous to the EPIDIDYMI in the Male.* By Professor KOBELT of Fribourg. (*Der Heben-Eierstock des Weibes, das Langstvermisste Seihenstück des Heben-Hoden des Mannes.* Heidelberg, in 8vo, 52 seite, Fig. 1847).

Notwithstanding the diversity of opinion entertained by embryological writers on the anatomical and physiological signification of the *Corpora Wolffiana* to the genito-urinary apparatus, they are nevertheless generally agreed, in considering these bodies as organs peculiar to the life of the fetus, and disappearing in both sexes without leaving any perceptible traces behind them. Hypotheses rather than facts have led physiologists to admit the *vasa aberrantia* of Haller of the epididymis in

man and the *organ* of Rosenmüller, as well as the ducts of Gaertner in the female, to be the presumed remains of the Wolffian bodies. Meanwhile it is possible to demonstrate anatomically the fact, that these products, which have been hitherto considered as temporary in both sexes, are not only continued during the whole duration of life, but further that they arrive at the greatest sexual maturity, and that they begin only to undergo changes, and wither, as it were, after the cessation of the genital functions, without, however, disappearing completely. The author gives in the following sentences the facts which he believes he has proved in the course of his Treatise.

1. In the earliest periods of intra-uterine life all individuals present a true sexual indifference, which hitherto has been admitted rather hypothetically than strictly proved.

2. This sexual indifference, or absence of sexual distinction, is constituted by the temporary co-existence in each embryo of all the elements constituting the generating organs of both sexes.

3. The generating gland, the point from which proceeds the whole of the sexual difference, may become either a Testicle or an Ovary.

4. By means of the excreting ducts of this generating gland, that is to say, by means of the canal of Muller in the female, and of the excreting duct of the *Corpus Wolffianum* in the male, and, on the other hand, by means of the mammary rudiments, which exist in all subjects, each embryo, in proportion as it becomes developed, may assume the attributes which characterise the one sex or the other.

5. The sexual difference is constituted and is pronounced by the increase of one of these conduits, and by the arrest of developement in the other.

6. In the male, the *Corpus Wolffianum* never entirely disappears in all its parts; but it constitutes in great part the epididymis, and this it does in the following manner.

7. Its *cul-de-sacs*, or blind tubes (*caeca*), constitute the eighteen or twenty of the *Coni vasculosi* of the epididymis. Subsequently these communicate freely, like *Vasa efferentia*, with the *Rete vasculosum Testis*.

8. The superior *cul-de-sac*, and the ampulla of the excretory duct of the *Corpus Wolffianum* are obliterated, or become, in consequence of their transformation hydatiform vesicles, situate upon or in the epididymis.

9. The inferior *cul de sac* of the *Corpus Wolffianum* disappear or are converted into the *Vasa aberrantia* of Haller, hitherto unknown in their origin and signification.

10. The excretory duct of the *Corpus Wolffianum* constitutes the canal of the epididymis and the *Vas deferens*.

11. The *ampulla* of the ducts of Muller becomes the hydatid of Morgagni; this conduit is at a later period itself found in the prominence of the anterior margin of the epididymis.

12. In the female, the *Corpus Wolffianum* never disappears in all its elements; but it concurs in the formation of the new body discovered by Professor Kobelt, and by him named PAROVARIIUM. This body is situate between the Fallopian Tube and the Ovary, in the ligamentous folds designated by the name of *Alue Vespertilionis* or Bat's Wings,

13. The middle *cul-de-sac* are transformed into the eighteen or twenty *Coni Vasculosi*, and penetrate by converging into the *Hilus Ovarii*. These are the analogues of the *Coni Vasculosi* in man.

14. The superior *culs-de-sac* also disappear, or at a later period constitute the hydatid vesicles so frequently observed upon the fringes of the Fallopian Tube.

15. The inferior *culs-de-sac* disappear. They correspond to the *Vasa Aberrantia* of Haller.

mon bile-duct (*ductus choledochus communis*); and the most minute dissection was unable to discover any thing which resembled a supplementary duct.

From this experiment the author deduces the conclusion, that the bile performs no essential part in digestion; that it ought to be considered as a refuse article, which, before being entirely expelled from the economy, performs still some services of very secondary importance, either by contributing with the other mucous fluids to convert fatty matters into an emulsion, or by protecting the intestines against the acrimony of chyme, the acid of which it partly neutralizes, and the progress of which it promotes by its nunctuous nature. This he thinks is sufficient to explain the constant situation of the excretory bile duct immediately below the stomach.

At a subsequent meeting of the Academy, on the 8th July, M. Semanas of Lyons communicated, *upon the Functions of the Liver during Digestion, and upon the uses of the Bile for Digestive Albumen*, a Memoir in which he opposed and questioned the correctness of the conclusions deduced by M. Blondlot.

M. Semanas does not dispute the correctness of the experimental fact itself; but he maintains that M. Blondlot had deduced illegitimate conclusions.

M. Semanas reproduces in this Memoir his ideas formerly published upon the subject of digestion, and gives a summary of these ideas in the following propositions.

1. Digestion properly so called, that is to say, abstracting from the processes relative to the combination of the alimentary articles, consists of two principal periods; *a.* intestinal or nutritive digestion; and *b.* hepatic or secretory digestion.

2. Hepatic or secretory digestion, the only one which the author considers in this Memoir, comprehends the preparation and absorption of albuminous materials.

3. The preparation of albuminous materials previously conveyed by the Portal Vein is performed in the interior of the liver by the bile which these materials find in this organ, which is united with them and alkalizes them with a view to their digestive absorption.

4. Digestive absorption of albuminous materials is performed in the interior of the liver by the agency of the hepatic lymphatic vessels.

5. From this it follows, that the liver may be the digestive organ of albuminous articles of food, and the bile, the principal action of which is consequently within the liver and not outside that organ, is the dissolving alkalizer of digestive albumen.

6. Lastly, the hepatic and cystic ducts are the evacuates of excremential bile, and very probably also the agents in the preparation of albumen, and of bile not excremential.

The reasonings and deductions of M. Blondlot have been here given as they are reported, without expressing any opinion as to their conclusiveness, and leaving that to be done by the opposing inferences of M. Semanas. It can scarcely, however, escape the notice of the most superficial reader, that M. Blondlot has not only, as M. Semanas represents, deduced from his single experiment conclusions not legitimate, but that he has strangely enough overlooked several important circumstances, and in his eagerness to prove that the bile is useless, he has reasoned in a manner which tends to make all reasoning on physiology of doubtful service.

It may be observed, in the first place, that though experiments on the lower animals are within certain limits important and necessary to

determine certain points, yet it is impossible to apply the results obtained by these experiments without any qualification or condition to what takes place in the human body. The bodies of animals suffer great injuries and mutilations, without inducing in them those serious and often fatal effects which follow in the human body. To maintain, that because an open fistula from the liver and gall-bladder carrying the whole bile without the body and entirely away from its usual route into the duodenum, does little or no injury to a dog, this fluid is of no use, and may be entirely dispensed with in a man, is a proposition so extravagant, that it can scarcely be received for one moment by the mind.

Without neglecting experiments, are we not to draw conclusions as to the utility or inutility of certain organs and their secretions from the phenomena daily afforded by the effects of disease. Does jaundice produce no bad effect upon the health of the sufferer? Is the man in whom the bile does not get into the duodenum in the same healthy state as the man in whom that fluid does reach the intestinal tube. Is *kirrrosis* a disease of no importance, and does it produce upon the digestion and the general health no unfavourable impression? Do people never die with jaundice or *kirrrosis*, that M. Blondlot thinks it a matter of indifference whether bile be secreted or not, or whether, when it is secreted, it is altogether of no moment whether it is mixed with the food or not. Until these questions be answered, without qualification or condition, in the affirmative, we think it will be safe for physicians to believe that the bile does perform some useful service, though it may not be possible to specify all the services which it performs.

Both in jaundice, in which the bile is secreted, but prevented from entering the duodenum, and in *kirrrosis*, in which it is probably not secreted in its proper form of bile, there is so much debility both of the alimentary function and of the general health, that it seems impossible to deny that the proper secretion of the bile is of the utmost importance both to digestion and nutrition, and consequently to the general health. The secretion of the bile appears to be serviceable in two modes; first, in removing from the blood principles which, if retained, must prove to be injurious; and, secondly, in effecting in the chyme, as found in the duodenum, certain changes, which, if not effected, render the chyme less nutritious, or at least less fit for the purposes of nutrition than it is in the normal state.

If it be objected that in *kirrrosis* the mischief appears to be done by the obstructed circulation in the liver, and by the dropsical accumulation in the abdomen; the answer must be, that dropsical accumulations from other causes in the abdomen do not kill with the same certainty or the same rapidity as that arising from *kirrrosis*, and that the injury inflicted on the digestive function and on the general health is invariably in the proportion in which the structure of the liver is changed, and consequently its function is suspended and interrupted. Scarcely any disease can be named, which so certainly disorders and impairs digestion and nutrition, and so certainly carries the patient to the tomb as *kirrrosis* in a perfect form and complete degree.

Further, if we take the story of the dog on which M. Blondlot performed his experiment, as it is recorded, it cannot be said that this experiment was without most hurtful effects. Surely neither the decaying health and pining of the animal during life, nor death itself can be termed trifling results; and to tell us that the animal died, without presenting any remarkable incident, after she had been wasting away for weeks at least, if not months, is a statement which, if M. Blondlot expects the profession to receive, he must possess a mind of most singular and unusual conformation. Even when the uneducated observe either

ment; a circumstance on which depends its faculty of forming fatty matters into an emulsion as completely as possible.

6. In the same animal the mixture of pancreatic fluid with bile, capable of being easily collected, in the state in which it flows into the intestine, possesses the emulsive and acidifying property, which is so remarkably possessed by the first of these fluids in a state of purity. But this property presents numerous variations which depend on the quantity, more or less considerable, of the pancreatic juice in the mixture, and which correspond consequently to the oscillations and the intermissions in the secreting action of the pancreas.

7. The abundance in the pancreatic secretion of different animals is not always in proportion to the size of the individuals and the volume of their pancreas. This defect, or rather this variability, in proportion, depends very probably in a great degree on this circumstance, that the formation and establishment of the pancreatic fistula do not offer in all of these animals the same difficulties, and induce not a perturbation equally sudden and considerable in the action of the gland.

8. Lastly, the pancreatic juice, by the constancy of its alkaline character, and the uniformity of its action upon fat matters, which it always converts into an emulsion and renders acid, in this manner offers exclusively some differences, characters, and properties, which are invariable in all the animals, in which its properties and actions have been studied.

II. MEDICAL PATHOLOGY AND THERAPEUTICS.

8. *Observations upon Cretinism, its causes, and the probable means of prevention.* By M. FERRUS. (A Memoir read at the ACADEMY DE MEDICINE on the 10th and 31st December 1850. Archives Generales, Fevrier et Mars, 1851, p. 231 and 353).

For some time past the subject of Cretinism has occupied not a little of the attention of the French physicians and the Members of the academy of Medicine. In the account given of the Pathological Anatomy of Cretinism in another part of this Number, it was mentioned that M. Ferrus had been making researches upon the nature and causes of this distemper.* The following is a summary of the Memoir read by this physician in December 1850.

After stating the situation long ascertained to be occupied by individual Cretins in different countries, and having established by descriptions, that it had been agreed to distinguish in Cretinism three degrees, represented by the three terms *Cretins*, *Sub-Cretins*, and *Cretinoid* persons, M Ferrus has brought out the shades which separate these different conditions and form the basis of this classification. He then directs attention to the topographical and physical characters of the districts most particularly ravaged by this distemper, and especially to his investigations in the mountains of the Valais and the Pyrenees; enumerates the alleged causes of the disease, and discusses by the method of approximation the question of the influence of waters, to which an official and public report had recently given considerable interest. On this point M. Ferrus announces the inference, that the constant presence of magnesia in articles of food and drink cannot be completely without effect in the production of Cretinism, and that the changes to which the waters are subjected in traversing cultivated districts had long appeared to him to deserve in this point of view particular attention. He deduced, however, from his own researches the conclusion, that the nature and composition of the waters, in all instances in which they were not presented in conditions too exceptional, were far from being capable of being considered as an exclusive cause, or even a very preponderating one, and that it was indispensably requisite to refer the origin of Cretinism to a simultaneous assemblage of elements capable in this concurrent manner of producing combined actions.

* See pages 336 and 345.

spot, and that he had ascertained by analysis that this spring-water also contained a notable proportion of magnesia.

On the other hand, M. Delafond, Professor of the School of Alfort, opposed this exclusive doctrine by facts derived from comparative anatomy and pathology. It is a fact, he argued, not to be disputed, that domestic animals partake of all the conditions of the existence of man, that they experience the same influence from localities, from kinds of food, and mode of habitation, which man does. Goitre, which has been met with in some animals, and is observed in Switzerland, in dogs and horses, has no where been seen among them as an endemic disorder. Meanwhile, animals are quite otherwise exposed from man to make use of water containing in solution calcareous and magnesian salts. Around the capital, at Vincennes, all the wells, the waters of which are drank by animals, contain a considerable quantity of sulphate of lime. Even the plants on which these animals are fed, saintfoin and clover, contain this salt in large proportion. Yet these animals never contract goitre, while we observe in them the formation of large vesical concretions, formed chiefly of carbonates of lime, and the developement of calcareous phthisis, the *pommelière*, or glandular disease, in which are sometimes found genuine deposits of calcareous salts. This, which is true of selenite or sulphate of lime is equally so of magnesia; for in animals, and particularly in horses, are found concretions and deposits formed almost exclusively of ammoniaco-magnesian phosphate, yet without goitre being observed to take place. The analogy, it must be admitted, is very remote. But it is impossible, that there can be, in this immunity from goitre enjoyed by the domestic animals, compared with the human race, any thing very extraordinary, and which it is difficult to explain, by admitting the influence of the waters used as drink as the sole and only cause of this distemper.

The nosological and pathological question of the subject was considered only by M. Rochoux and M. Baillarger. The discussion turned more particularly upon the nature of the disease and the pathological changes with which it is associated. M. Baillarger looked to the point of the analogies and the differences between Cretinism and Idiocy, comparing the differences which have been stated to subsist between these two disorders, he aimed at this result, that these differences had been imperfectly estimated, or were insufficient to distinguish these two disorders in the Nosological Table. He therefore infers the identity of the two disorders. His conclusions were verbally the following.

1. Cretinism and Idiocy are essentially characterized by an arrest of developement and of intelligence.

2. In Cretins, and in idiots, this arrest of developement must be referred to the same anatomical lesions, that is to say, to anomalies and lesions in organization.

3. Hydrocephalus is found accidentally in Idiots and in Cretins; but cannot in the present state of science serve as the foundation of a definition of Cretinism, and to be regarded as the constant and essential anatomical character of this distemper.

4. Keeping in view the differences which subsist between Idiots and Cretins, considered in a general manner, these differences can be regarded only as accessory and insufficient to distinguish Cretinism from idiocy.

5. The application of the law to prevent the marriage of Cretins would be a measure very difficult to be carried into effect, but particularly, it would be a measure inefficient to prevent the propagation of hereditary Cretinism.

6. The most practicable means probably for obtaining this result would be; 1. to determine correctly the conditions of health, which in

9. *The Spontaneous Development of Air in the Blood as a cause of Death.* By M. DR DURAND FARDEL., (Archives Generales, Proceedings of Academy of Medicine, January 1862. P. 98.)

A lady, aged fifty-six, of tall stature, and considerable corpulence, came to Vichy, accompanied by her husband who was labouring under gravel. This lady in apparent good health, complained sometimes not of palpitations at the heart, but of a little difficulty in breathing, and wished like many persons to take baths, for which purpose she obtained the authoritative sanction of the physician, under whom was her husband.

On the 20th of July she went to the bath-establishment to take her second bath, at eight o'clock in the morning. She was well the previous evening, had dined in the usual manner, and had slept well. The only uneasy feeling which she had was shortness in breathing; she was obliged to remain seated before she entered the bath. Half an hour, she requested to be taken out. She was in a bad condition; and when she rose from the bath to change linen, she appeared to be in agitation, and complained of oppression. She then went out and sunk down on a seat, with panting breathing, and unable to speak. When M. Durand came five minutes after, she was lifeless. The face was quite void of colour; the head fallen back on the chest and moveable; the lips blue; the features calm and undisturbed, without foaming at the lips; the limbs flaccid and insensible. The pulse had ceased at the wrist and heart. The pupils were dilated and moveable,* the conjunctivæ insensible to touch.

Although these phenomena left no doubt upon the cessation of life, the median basilic vein was largely opened, and there flowed from it a little blood, not black, but violet-coloured, frothy; that is to say, accompanied with air-bells of unequal size, which at the same time escaped from the vein. For more than fifteen minutes, M. Durand employed unavailing attempts, tickling the uvula, presenting ammonia to the pituitary membrane, &c. During this time the frothy blood continued to trickle from time to time under the influence of pressure made on the fore-arm from below upwards. Once the jet was discharged forcibly, and continued for five or six seconds, as if it had been propelled by a bell of air which might be expanded in the interior of the vessel. A small quantity of white foam then appeared on the lips.

The body inspected twenty-two hours after death presented the following phenomena.

No trace of decomposition; some discoloured marks only upon the dependent parts of the trunk and extremities.

The heart was very much enlarged. The right chambers were distended by blood entirely liquid, violet-coloured rather than blackish, as if syrupy, very frothy; the bells of air which it contained were some of them very numerous, of the size of pin-heads, others fewer of the size of garden-peas.

When pressure was made upon the course of the two *Venæ Cavae*, the blood which flowed into the right auricle was frothy like soap water. The walls of the right chambers of the heart presented superficially a violet-coloured staining. The left chambers were quite empty of blood, and void of colour. The left ventricle was considerably hypertrophied. No change had taken place in the orifices of the heart, nor at the beginning of the aorta.

The lungs filled the chest and presented a small number of adhesions, without any trace of emphysema. Their colour was a little reddish externally, deeper coloured in the interior, where they presented traces of bloody congestion to a considerable degree, without infiltration of blood. There

* * For moveable we suspect it ought to be motionless.

was frothy congestion moderately abundant in the depending portions. A little whitish frothy mucus was found in the bronchial tubes.

The whole abdominal venous system was gorged with violet-coloured frothy blood, and numerous bells of air were observed in the blood of the splenic vein and portal vein.

In the abdominal organs, there was bloody congestion to a considerable degree in the liver, the spleen, the kidneys, and injection of the veins of the omentum and mesentery. The omentum was much loaded with fat.

The stomach was large and voluminous, containing about half a glass of clear colourless mucus. The intestines were not opened. The gall bladder contained a moderate amount of blackish syrup looking bile.

In the brain was the same degree of bloody congestion as in the other organs; a little liquid blood, not frothy, was found in the sinuses of the *dura mater*. The brain and origin of the spinal chord were a little injected with blood. No air was observed in the vessels.

M. Durand-Fardel thinks, that in this case we must admit the spontaneous evolution of gas during life in the venous system, probably in consequence of a spontaneous alteration in this fluid, neither the nature nor the cause of which, however, is known.

10. Caustic-Holders made of Gutta-Percha. (In a letter to the Editor.)

SIR,—It is well known that the sticks of nitrate of silver corrode, blacken or injure the various kinds of caustic-holders, except those made of silver; these last are expensive, and after some time, even they are somewhat injured.

I have of late employed gutta-percha for this purpose; a piece of a "hearing-tube" may be used. Gutta-percha being incorrodible and cheap, seems better adapted than any other substance for this object.

If I am correct in my supposition that this proposal has not been previously made, your insertion of this note will much oblige.—Yours, &c.,

Prince-Edward Island,
1st March 1852.

THOMAS STRATTON, M.D., R.N.

III MEDICAL SURGERY.

11. *Instance of Foreign Body impacted in the Orifice of the third Branch of the Right Bronchus, with Remarks.* By JOHN GREGORY FORBES, Fellow of the Royal College of Surgeons, England. (*Medico Chirurgical Transactions.* Volume xxxiii., p. 1. London 1850.)

In the forty-second volume of this Journal, was given a series of cases illustrating the nature and effects of the accidental introduction of Foreign Bodies through the Larynx into the Wind Pipe and Bronchial Tubes; with references to other cases either in the same journal or in other writings. One of the objects of the writer was to correct what he thought he perceived to be certain erroneous notions prevalent upon the situation of these bodies: namely, that they are situate not in the lungs, properly so called, but in one or other of the upper bronchial tubes upon the right side; that in this situation they give rise to symptoms, in some instances very intense, of chronic *bronchitis*, so intense indeed as completely to imitate the symptoms of pulmonary consumption, and to have been mistaken for that disease; that, nevertheless, under certain favourable circumstances of shape, figure, and weight of the body so conveyed into the *bronchi*, it may at length be expelled by coughing;

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and the individual may make a recovery temporary and partial, or permanent and complete.*

The nature of this species of bronchial inflammation was considered in the first edition of the *Pathological Anatomy* of Dr Craigie, and in the *Practice of Medicine* of the same author; and in the second edition of the *Pathological Anatomy* was given a tabular view of twenty-four well authenticated cases of the incident, and the inferences which these cases seem to warrant.†

In the inference, number eight, it is stated that "In certain cases, in which the mechanical configuration of the body is unfavourable for detachment and expulsion, the bronchitic symptoms are liable to be extended to the lung, in which suppuration is caused, and to the pleura, in which effusion of lymph and purulent matter is introduced."

This is what takes place in the most intense and long continued cases of the incident, whether the body is expelled and recovery is effected, or the body remains and death ensues.

Besides the instances recorded in the works referred to of this extreme form of the lesion, a pointed example is recorded by Mr W. G. Carpenter, in *Guy's Hospital Reports*, (volume seventh, p. 353. London, October, 1842), and of which an abstract is given in the seventy-first volume of this *Journal* p. 234, January, 1849.

An example of the unfavourable form of the incident is recorded by Mr Forbes; and it has induced this gentleman to consider carefully the circumstances indicating and contraindicating the operation of opening the windpipe.

The inquiry involves not merely the question of operation and its admissibility. The operation is in general perfectly practicable, so far as making an opening into the wind-pipe is concerned. But it by no means follows that when the opening is made, the body can be extracted or is removable. Thus in the noted case of Sir Isidore Brunell, in whom the operation was performed for the removal of a coin, it was found impracticable to extract the coin by forceps; but it was afterwards expelled by coughing, or rather by the effect of gravitation. In most of the cases hitherto tried, the introduction of the forceps is followed by violent coughing, and the appearance of suffocation so threatening, that it is impossible to persevere. In certain cases, however, though the body cannot be reached or seized by the forceps, it is eventually expelled through the artificial opening.

The following are the details of the case given by Mr Forbes, in which it was considered unsafe or inexpedient to attempt the operation.

Mrs W., aged 46; of pale complexion and thin person, applied at the Western General Dispensary on the 11th of May 1849, stating that on the previous day, at 2 P. M., whilst eating some broth, a piece of solid matter, which she believed to have been 'bone covered with gristle,' passed into the windpipe. She was immediately seized with spasmodic cough and threatened suffocation; 'her face became black, and water ran from her mouth,' and it was some minutes before she recovered herself. She fancied that at first she felt it sticking across the windpipe, but that she squeezed it lower down with her fingers. Soon after the accident happened, a probang was passed into the stomach, but no obstruction was found.

* *Edinburgh Medical and Surgical Journal*, vol. xlii., p. 103. Edinburgh 1834.

† *Craigie's Pathological Anatomy*, 2d edition, p. 590. Edinburgh and London, 1848.

count it is to be hoped, that giving the report such a degree of publicity as we can, may have beneficial effects.

On the other hand, there are, relating to this and all questions placed in the same circumstances, difficulties almost insurmountable to any good results being obtained. The administration of the laws relating to quarantine is entrusted to a set of persons, whose ignorance can be equalled by nothing but their obstinacy. To these persons offices of rank, authority, and considerable emolument are assigned; and, indeed, their authority rests upon, and their emoluments are derived from, maintaining the opinion and the principle, that yellow fever is a contagious disease, and can be imported from the West Indies and the south of Spain. So long as these gentlemen are in the receipt of large sums of money for maintaining these opinions, it is altogether unreasonable to expect, that they will ever relinquish these opinions, or be made to admit that yellow fever is not contagious, and cannot be imported. To expect such a concession, is to require too much from such frail materials, as those of which men and physicians are composed. We, therefore, regard the whole matter of controversy as nearly altogether useless and unprofitable. Those who can be convinced, have not the power to abrogate or alter the quarantine laws; and those whose interest it is to maintain the present state of things never will be convinced. We believe, therefore, that a long time of ignorance, presumption, and conceit, as well as avarice, must yet elapse, before any reports from scientific bodies shall be of essential service.

13. *Establishment of a Lectureship on Military Surgery in Dublin.*

Directions of H. F. I. C., that candidates for admission into their service shall attend Lectures on Military Surgery.

In the fortieth volume of this Journal, (p. 456), in giving a view of the History of Military Medicine and Hygiene, we were led to observe that the chair of Military Surgery in Edinburgh, which has been the earliest establishment of the kind in Europe, may become the model of similar establishments in different countries. It is scarcely necessary to explain, that this was said in reference to the duties of the chair being chiefly directed to the Department of Military Medicine and Hygiene in general, as elucidated in the article in that volume. A long time may perhaps elapse before this prediction may be fully realized; and so far as it is connected with the exigencies of a state of war, we have little desire to see it realized at all, or in any way.

War, however, it is remarked, in the same article, is a necessary evil; and in the present state of the world, it is not more likely to be dispensed with than at any former period. It becomes therefore both nations and individuals to be prepared against its unavoidable casualties and calamities. If one institution be more necessary than another, it is that, the object of which is to diminish the number and mitigate the severity of the various evils necessarily attendant on those engaged in the duties of the Naval and Military service.

The most important and serviceable manner in which the Medical Officers of the army and navy can be employed, is in understanding perfectly and performing diligently, the duty of preserving the health of the men committed to their professional charge. It is both easier and more economical to preserve health, whether among individuals or bodies of men, than to restore it when impaired; and prophylactic or preventive medicine (*Medicina Phthastica*) is, though little understood, by far the most useful division of the art. This department, therefore, which allies itself with the general movement for sanitary improvements and civil hygiene, ought to obtain that serious attention, to which it is most justly entitled, in any course of instruction prescribed by public boards for public medical

officers. We must accordingly be permitted to maintain, as strongly as ever, the necessity of instructing those who are to have the medical charge of soldiers and seamen, in all those branches of knowledge, the object of which is to preserve health, and to repel and prevent the attacks of disease, especially when epidemic or endemic, or in any mode dependent upon the operation of preventable causes. Our military and naval history is full of records of ruinous and discreditable losses of men, from ignorance of, and inattention to, the known rules of public Hygiene. An encampment or bivouac made in an improper and unhealthy position, an hospital erected in a bad situation, and without adequate regard to ventilation, have in too many instances caused more mortality than the fire and sword of the enemy.

All competent judges will admit, that it is a movement in the proper direction, that a Lectureship of Military Surgery, is established in the City of Dublin; and that attendance on the Course is recommended in the statutes promulgated by the University in November 1851. The only modification we should desire to see, for reasons fully stated before, is, that it be a Lectureship of Military Medicine and Hygiene. But in the meantime the act is a sort of initial step of what ought to be done with the purposes already stated. Nor is it of less consequence, that the Hon. East India Company have evinced a desire, that the persons employed as their medical officers shall have devoted attention to this branch of medical education. Certainly it would be difficult to name any place or any situation, in which such knowledge must be more requisite and more beneficial, than in those extensive regions, which are subject to the sway of the Honourable Company. We only add that this measure may probably lead to the establishment of a Lectureship in London; and whether this takes place or not for the surgeons of the E.I.Co's. service, we think that the time is approaching when public opinion will require that medical practitioners shall study the subject of Hygiene with sufficient attention, to enable them to give sound advice on the preservation of health and the prevention of the causes of sickness.

University of Dublin.—Diploma in Surgery.—Resolved by the Pro-vost and Senior Fellows :—That a diploma in surgery be given to such students as are matriculated in medicine, and have completed at least one year in arts, on the following conditions :

1. To complete one year in arts, it shall be necessary to have answered at least one examination, subsequent to the Junior Freshman year; or to have completed the Junior Freshman year only, by passing the Michaelmas examination of that year, and keeping one previous Term, either by lectures or by examination.

2. Students who have not passed an examination in the Senior Freshman year will be required to attend one course of lectures in logic.

Students who have not passed the junior sophister year of the undergraduate course will be required to attend one course of lectures on mechanics, with the assistant to the Professor of natural philosophy.

3. Students so qualified will be admitted to examination for the diploma in surgery, as soon as they shall have completed the prescribed curriculum.

4. This curriculum shall extend over a period of four years, and shall comprise attendance upon the following courses of lectures in the school of physic in Ireland :

Anatomy and Physiology,	- - - -	Three Courses.
Demonstrations and Dissections,	- - - -	Three Courses.
Theory and Practice of Surgery,	- - - -	Three Courses.
Practice of Medicine,	- - - -	One Course.
Chemistry,	- - - -	One Course.
Materia Medica,	- - - -	One Course.

from the Royal College of Surgeons of London, or of the Colleges of Surgeons of Dublin or Edinburgh, or of the College and University of Glasgow, or of the Faculty of Physicians and Surgeons of Glasgow, either of them will be deemed satisfactory as to his knowledge of surgery, without any further examination. He is also required to produce a certificate, from the cupper of a public hospital in London, of having acquired, and being capable of practising with proper dexterity, the art of cupping.

Qualification in Physic.—The Assistant Surgeon will also be required to pass an examination by the Company's examining physician in the practice of physic, in which examination will be included as much anatomy and physiology as is necessary for understanding the causes and treatment of internal diseases, as well as the art of prescribing and compounding medicines; and Dr Scott will then require him to produce satisfactory proof of his having attended at least two courses of lectures on the practice of physic; and, above all, that he should produce a certificate of having attended diligently the practice and clinical instruction of the physicians at some general hospital in London for six months; or at some general hospital in the country (within the United Kingdom) for six months, provided such provincial hospital contain at least, on an average, one hundred in-patients, and have attached to it a regular establishment of physicians as well as surgeons. It is also expected that the assistant surgeon shall produce a certificate of having diligently attended, for at least three months, the practical instruction given at one of the asylums for the treatment of the *insane*, and at one of the institutions or wards of an hospital especially dedicated to the treatment of *ophthalmic* disease. He will also be required to attend a course of lectures on the principles and practice of *military surgery*, if such a course shall be given at the place at which he has been educated. No attendance on the practice of a physician at any *dispensary* will be admitted.

The assistant surgeon is also required, as a condition to his appointment, to subscribe to the Military or Medical and Medical Retiring Fund at his respective presidency, and also to the Military Orphan Society, if appointed to Bengal.

The assistant surgeon is required by resolution of Court of the 21st May 1828, to apply at the Office for Cadets and Assistant Surgeons for his orders for embarkation, and actually proceed under such orders within three months from the date of being passed and sworn before the committee for passing military appointments; he will then be furnished with an order to obtain the certificate of his appointment, signed by the secretary, for which he will pay a fee of L.5 in the secretary's office.

14. In the Foreign Papers is announced the death of Vincent Priessnitz, at Graefenberg, on Saturday, the 29th November 1851, at the age of fifty-two years.

He had been unwell for some little time, though on the nature of his ailments no exact information is given, or could be obtained, in consequence of his rooted dislike to the regular practitioners of medicine, to none of whom he would apply. On the morning of the 29th November, Priessnitz rose at an early hour, as was his custom; and was preparing to go through some of his duties. He felt extremely cold, and, according to the account, either gave directions for a fire, or allowed one to be prepared. Some of his friends suggested that he should send for a physician; but he replied that it was of no use, yet gave a sort of assent. The sensation of coldness continued to increase, and Priessnitz breathed his last on the afternoon of the same day.

He appears not to have been visited by any physician. Some rumour there was of his having symptoms of hydrothorax, or some similar disease of the thoracic viscera. But upon this point nothing accurate is known.

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